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# A HISTORY OF ASTRONOMY

# A HISTORY OF ASTRONOMY

Soli Deo
Glori
Narrant
DEI
Gloriam
Magna
Opera
DOMINI
Omnis Spi-
ritus lau-
det Domi-
num.
Non Nobis, non Nobis, sed Soli
Waltherus
Tycho
Ulug Beigh
Timorchan
Andr. Stech pinx.

# A HISTORY OF ASTRONOMY

BY

WALTER W. BRYANT

B.A., F.R.A.S., F.R.MET.SOC.

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[illegible] OF THE ROYAL OBSERVATORY, GREENWICH

METHUEN & CO.
36 ESSEX STREET W.C.
LONDON

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TO MY WIFE

# PREFACE

THE aim of this book is to set before the general reader an account, neither too long nor too technical, of the History of Astronomy, tracing its progress from early times, past the great names of celebrated pioneers down to our own day, when the development is so wide that the biographical element has perforce to give way. Of the many writers whose works I have consulted during the compilation of this volume, I feel bound to make special reference to the late Miss Agnes Clerke, the value of whose careful work to any one making researches in the same direction can hardly be over-estimated. My special thanks are due to the Astronomer Royal; to Sir David Gill, Monsieur Loewy, Professor Frost, Professor Barnard, Professor Küstner, Professor Weinek, and others; and to the Royal Astronomical Society for permission to use many of the illustrations; and to Mr P. Melotte, of the Royal Observatory Photographic Staff, for much assistance in selecting and providing them. I take this opportunity also of expressing my indebtedness to several of my colleagues at the Royal Observatory, and especially Messrs Lewis and Crommelin, for valuable hints and corrections; and also to my friend, Mr R. A. Streatfield, who has not only edited my manuscript, and corrected my quotations, but has also given general assistance in the preparation of the scheme of the book and of the illustrations, in respect of which my publishers have also taken a great deal of trouble, which I gratefully acknowledge.

OLD CHARLTON, S.E.,
*August* 9, 1907.

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# A HISTORY OF ASTRONOMY

## CHAPTER I

### EARLY NOTIONS

AMONG the earliest traditions of every primitive race may be found traces of attempts to explain the elementary phenomena of the heavenly bodies. It could hardly be otherwise, since the succession of day and night, of winter and summer, and the phases of the moon could not have passed unregarded, even in the most uncivilised times. Few, if any, other sciences can claim such high antiquity as astronomy, the elementary stages of which would seem almost coeval with the human race. This primitive astronomy, however, can hardly be regarded as a science, and we are without any authentic data as to the course and extent of the progress made by any of the ancient races from the simple noting of phenomena, to which Plato, criticising Hesiod, denies the right to the name of astronomy, towards the determination of the laws which govern them. Plato's criticism goes plainly too far, as observations are quite as indispensable as theory, but it leads us to infer that the theory in his time was not regarded as of very great antiquity.

It is probably safe to assume that the traditional cradle of nations, the Iranian plateau, was also the cradle of knowledge, that every successive migration carried some of its ideas into successive regions, and that the highest development of its science was reached by those who were last to leave.

The order of these migrations is a matter of general history and of some uncertainty, but there is little doubt that the astronomy of India, China, and Egypt was inspired by the sages whose more direct and legitimate successors were the Chaldean priests.

The process of evolution would begin simply enough. The common phenomena of dawn, sunrise, day, sunset, dusk and night would first attract attention, and their regular succession be immediately noticed. The varying darkness depending on the light of the moon, as well as the changes in the time of rising and setting and of the apparent shape of that body, would give another longer measure of time, so that besides the day or day-and-night, time came to be measured by months or moons, a mode of reckoning still found among savages. Soon it would be observed by noting the varying length of the day (since we are dealing with the temperate regions as the primitive abode of man), or the direction of the sun at rising and setting, or the length of midday shadows, that the sun's path also changed from day to day, and that he rose and set farther and farther north from winter to summer, reaching in consequence a higher elevation at noon, and reversed the process from summer to winter. This cycle, obviously coinciding with the course of nature in the fields and woods, gave another and a more important time-unit.

It is not so easy to fix the length of the year, however; and the husbandmen, to whom it was of most importance, soon learnt to rely on a class with greater leisure to fix for them the seasons with greater accuracy. The priests naturally claimed and exercised this office, magnifying its importance, elaborating ceremonial sacrifices, and thus strengthening their ascendency over the ignorant.

They must soon have discovered that there is not an exact number of days in a month, or in a year, or of months in a year, and have set themselves to systematic night observations to determine these relations with greater accuracy; at the same time devising various schemes to evade the difficulty, especially that caused by the length of the month.

They would notice that the moon followed roughly a certain path among the stars, and that the sun's path was nearly identical, and that at the same season of the year the sun's place in that celestial track was always the same. So that zone of the heavens came to be regarded as distinct, and the configurations of the stars in different parts of it were associated with different seasons, and divided into groups called constellations, and given names in order to define the sun's position, that is, the time of year. This zone has long been called the Zodiac.[1]

---

[1] The name signifies live things; the only sign we use not answering this description is Libra, which was formerly the claws of Scorpio.

So far, there is reason to suppose a general agreement among all primitive races, but the next step involves great uncertainty. Disregarding the "lunar mansions," which would divide the zone into about twenty-eight portions, the obvious division for the sun would be into twelve portions, each corresponding with fair approximation to a month. As a matter of fact this was done in nearly every case of which we know anything. The zodiac, then, was divided into twelve portions, and the sun connected successively with each in his course through the year. But the question is, "How was he so connected?" and it is here that practice probably differed. Since the sun and the stars could not be seen together (except occasionally during an eclipse, when in early times people were probably too frightened to take observations), some indirect method must have been used. It will be easily understood that the sign opposite to that in which the sun is situated will be towards the south at midnight, will rise at sunset, and set at sunrise, and so could be determined with fair precision; so that from a zodiacal map the sun's approximate position among the stars would be at once inferred. But there is strong reason to suppose that this was not the method in general use. In any case the idea of midnight involves the use of some sort of clock, and here our inferences are valueless. It is fairly certain that some early astronomers made observation of the last conspicuous star rising just before the sun. It is also very probable that others made use of the new moon as a link, and associated the sun, by its means, with the constellation setting just after sunset. Of these two methods one is usually associated with Egypt, the other with Chaldea, and it may easily be seen that from either of them great uncertainty is introduced into the solution of the problem so many have attempted, namely, to identify ancient dates by referring them to supposed zodiacal positions of the sun, or heliacal risings of stars, especially the former. For example, many centuries ago the sun was in the constellation of the Bull at the vernal equinox (that is the time when in his northward journey he has reached the half-way house, where, rising due east and setting due west, he makes the lengths of day and night equal). If, however, an inscription associates the sun with the Bull at the vernal equinox, it is quite possible that there is a moon also associated, and that it is the young moon that was in Taurus and not the setting sun. Hence an uncertainty of over 2000 years in the date.

But our primitive astronomers found other celestial objects travelling in the zodiac besides the sun and moon; four very obvious ones, and a fifth whose discovery is pre-historic. Two of these, now known as Venus and Mercury, never appear far from the sun, while the other three do. These were recognised as belonging to one class, and called wandering stars or planets. Their courses are performed in very different times, varying from about three months to thirty years, but their motions would be known on the average with fair accuracy soon after systematic observations began, and the priests were not long in seizing the opportunity of elaborating a new branch of mythology associated with them.

Eclipses of sun and moon, though the natural dismay they caused would be exploited by the priests for their own purposes, must soon have provided the priests themselves with an eclipse cycle; or at least with a more accurate value of the length of the month, and would be recognised as valuable phenomena for both reasons.

It is not necessary to rely on the Jewish tradition that the lives of the early patriarchs were specially prolonged in order to enable them to determine the greater astronomical periods (they specially mention the "great year" of 600 years, after which they supposed the configurations of the planets to be repeated in the same order, as having become known in this way), for an ordinary generation of systematic observation would have yielded nearly all the information they can be proved to have possessed.

Comets probably perplexed even the priests, but the number visible in a generation could not have been large, though possibly it was once greater than it is now, since more than one notable periodic comet appears of diminished splendour at each successive return.

After mapping the zodiacal constellations, it would be a simple step to fix a few groups in other parts of the sky, either in the south, where their risings and settings would give an indication of the time of year, in addition to those of zodiacal ones, and in some cases with greater value owing to the greater brightness of some of the stars; or in the north, where many of them would not set at all, and where those whose motion was slowest would have a special value for the primitive sailor, a value that is not yet lost.

So far, there is little difference between the astronomy of the nations of the East, Chaldea, Egypt, India and China, and that of the Incas, the Aztecs, the Druids, or the South Sea Islanders, or any other primitive race. The nations of the East, however, demand a short chapter to themselves.

# CHAPTER II

## THE EASTERN NATIONS OF ANTIQUITY

WE may safely put aside the improbable tradition mentioned by Josephus that the immediate descendants of Seth were comparatively advanced astronomers, who recorded the state of the science on monuments intended to survive the expected Deluge, one of which is said to have not only done so, but to have been still existing in Syria in the time of that credulous historian. But it will be as well briefly to glance at the pretensions of the four Oriental nations referred to in the last chapter, in so far as they claim priority in scientific astronomy. Chinese records tell of a conjunction of five planets about 2500 B.C., and of a solar eclipse in Scorpio 2159 B.C., about which it is said that the government astronomers, Ho and Hi, were beheaded for failing to predict it. They further relate that as early as 2857 B.C. the emperor recommended the study of astronomy, and made it an important subject, and that a later emperor caused complete astronomical records to be destroyed; but there is internal evidence of the untrustworthiness of such annals. Even in historic times the records are often entirely absurd, as, for instance, those commemorating respectively a cloudless night without stars and a star as large as the moon, to say nothing of the fact that there is a gap of 1383 years between the first two eclipses, and that none of them, with one doubtful exception, can be identified earlier than the time of Ptolemy. We can admit that the early Chinese astronomers observed eclipses from very early times, but it is certain that we owe them practically nothing beyond a determination of the "obliquity of the ecliptic" about 1100 B.C. at Lo-yang, which confirms the secular diminution of that obliquity. The fact appears to be that the Jesuits who settled at Pekin in the seventeenth century taught the Chinese more astronomy than they ever knew before, but that the wily Celestials bluffed the Jesuits by producing records of past occurrences concocted by calculating backwards, or misled them by falsifying the dates of real ones, expressed in characters unintelligible to the foreigners. This seems to be the simplest way of reconciling the obvious sincerity of the belief of the Jesuits with the extreme improbability of its justification.

See p. 15.

A parallel case (totally irrelevant, however), I heard, from an old military officer who was employed in Syria at the time of the trouble about the Druses. When the War Office objected to the amount of his claim for horse-hire and demanded vouchers, after first suggesting that he ought to have taken a cab, he turned out from his baggage a number of scraps of paper, washing-bills possibly, with some sort of marks on them, and forwarded them in the perfectly justifiable belief that an official who suggested cabs on Mount Lebanon would most certainly be nonplussed by the hieroglyphics.

Indian astronomy is different in that it possesses a system of its own, with tables and rules for calculation, the basis of which claims to be a conjunction of the sun, moon, and planets in 3102 B.C. The tables give fairly good results, and are evidence of considerable advance in science, but as the conjunction in 3102 B.C. certainly never took place, the antiquity of the tables is open to grave doubt, and it is confidently asserted by some that they are not older than the Mohammedan invasion of India. Whether they date from either of these epochs or from some intermediate one, it is quite likely that they were imparted to the Brahmins by some other people, and adapted by them to their peculiar methods. And even admitting the antiquity claimed for India as the cradle of all the arts and sciences, it is certain that astronomy, as we understand it in England, did not come from there, or receive any aid whatever from that source.

Egyptian astronomy, again, lays claim to high antiquity, and the evidence freely adduced here, though almost entirely circumstantial, is at any rate interesting. Much has been written as to the astronomical meaning of the Great Pyramid, and it is often assumed that its age can be certainly fixed by the circumstance that at some distant period a bright star, now some distance from the Pole, was near enough to be used for the Pole Star, and that its altitude at one of its culminations would then have enabled it to be observed through a long shaft-like opening in the northern face of the Pyramid. It is also freely asserted that Egyptian temples were oriented to the rising-points of certain stars, and if sufficient latitude be given for an inexact observation of a star, some star can usually be found which at some distant date might have answered the purpose. But in general it seems there is no direct evidence to connect any particular temple with any particular star, and even in the case of Sirius, whose identification in Egyptian mythology is less uncertain, there remains the doubt as to the altitude at which the observation would be taken. Another claim on behalf of Egypt is the tradition of the Greeks as to the long records of Egyptian observations. But as this is only a tradition, and as none of the Greek astronomers made use of any such observation, we may safely conclude that none were to be found, and that either none were taken or recorded, so that the tradition can only be explained on the "pride of race" hypothesis (since the Greeks claimed an Egyptian origin), or that the records had perished, in which case they cannot be said to have contributed anything more than Indian or Chinese annals to the progress of astronomy.

There remains Chaldea, and here we are on rather firmer ground; for besides the probability inferred from its proximity to the traditional cradle of the human race, there is the undoubted fact that the earliest observations used by Ptolemy were three eclipses observed at Babylon in 721 and 720 B.C. There is, moreover, indirect evidence that in those regions the constellations as known to the Greeks and (with various additions) still in use were first mapped out, as all of them would have been visible there about 2000 B.C., while some constellations, visible further south, as in India or Egypt, were not named so soon. This hypothesis rests upon the established fact of "precession."[1] Be that as it may, the progress made in astronomy by the Chaldeans and Babylonians was not very great, as they made very little advance in theory, but reasoning from analogy which associated certain celestial phenomena with certain facts of common life—seed-time, harvest, equinoctial gales, and so on—they were diverted from the track of science, and endeavoured to connect other less periodic phenomena with other less periodic and less certain facts of life. Hence arose the so-called science of astrology, which though long ago discredited, still finds some votaries. It was, however, necessary for the purposes of astrologers to have a fairly correct idea of the motions of the planets and the cycles of other celestial phenomena, so that observations were still taken, which might have had great astronomical value. It is, moreover, generally conceded that the Oriental mind, though capable of great achievements in the manipulation of numbers, was not adapted for the kind of philosophical speculation in which the Greeks excelled, and that observations would in Eastern hands have produced very little theory.

---

[1] A slow motion of the earth's axis, which is not quite fixed in direction in space, but completes a cycle of change in about 26,000 years, thus also varying the portion of the sky visible in any latitude.

The Babylonian priests are said to have told Alexander the Great that their astronomical records went back 403,000 years. A very plausible explanation of this is that at the rate of diminution of the obliquity of the ecliptic which they might have ascertained, on the erroneous assumption that the rate was constant, they had worked back to the date at which the obliquity would have been a maximum, *i.e.* a right angle, which has been verified within a few years of the vast period given. If this is the case, there is a family likeness between this and the planetary conjunctions of the Indian and Chinese systems.

See p. 15.

But we must hasten on to still firmer ground.

# CHAPTER III

## THE GREEKS

THALES of Miletus is generally called the founder of Greek astronomy. He taught the Egyptians to measure the heights of their pyramids from their shadows, a fact which throws an interesting sidelight on the elementary state of Egyptian science in his day, though possibly the actual pyramid builders knew very much more. He taught also that the stars shone by their own light, but that the moon received light from the sun, that the earth is spherical, and that the year contains 365 days. But he is chiefly remembered in connection with the prediction of an eclipse of the sun which put an end to a war between the Medes and Lydians. As, however, he only fixed the year in which it was to take place, the fact that this was regarded as a marvellous achievement shows plainly how elementary the state of the science was in his time. Various dates have been assigned for this eclipse, ranging over some forty years, but it probably took place on September 10, B.C. 610. Soon after this time the gnomon, a vertical pillar, whose least shadow marked the time and altitude of the meridian sun, and from whose least and greatest midday shadows the obliquity of the ecliptic could be calculated, was first used in Greece.[1] We also now first hear of a Greek sundial, but both of these ideas may have been imported from Babylon. The most commanding figure of the century after Thales was Pythagoras, who travelled in the East and ultimately settled in southern Italy. He first suggested, without apparently regarding it as more than a speculative hypothesis, that the earth revolved round the sun, a doctrine occasionally taught by some of his followers, one of whom, a century later, taught also that the earth revolved daily on its axis. The school of Aristotle soon discarded the happy suggestion of Pythagoras, which was not revived for many centuries. It is claimed, however, that a century before Aristotle, Democritus of Abdera, "the laughing philosopher," held far juster notions, but his numerous works have perished.

Meton and Euctemon at Athens observed the summer solstice of 432 B.C., the earliest reliable observation of the kind, if we regard those of the Chinese as doubtful. But their greatest achievement was the "cycle of Meton." A luni-solar period, after which the sun and moon would be in the same positions relatively to the stars, was of great importance in fixing festivals, and had long been sought. The eclipse period of about 6585 days, or 223 lunations, a few days more than 18 years, was known. This sufficed to predict eclipses, but bore no relation to the solar year. It is supposed to have been discovered by the Chaldeans, and is known as the Saros. Meton, however, hit upon a period of 235 lunations or 19 years approximately, which would bring the sun and moon within a little of the same position relatively to the stars.[1] This discovery was hailed with acclamation at the Olympic games, and the cycle commencing July 16, 433 B.C. was adopted in Greece and its colonies. It was inscribed on brass in golden letters, and is still in use in determining Easter, the golden number being the number of the year in the cycle of Meton.

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[1] The obliquity of the ecliptic. If the sun were always in the equator the equinox would be perpetual instead of semi-annual; but as this is not the case, the apparent motion of the sun takes place in another plane, cutting the equator at the equinoctial points, and the inclination of this plane (called the ecliptic, because eclipses only occur when the moon is in or nearly in the same plane) to the equator is called the obliquity of the ecliptic, and is half the difference of the meridian altitude of the sun when greatest (at mid-summer) and least (at mid-winter).

His system of the calendar required the 235 lunations to consist of 125 months of 30 days, and 110 of 29 days,[2] and the 19 years to consist of 12 years of 12 months, and 7 of 13 months. As this was such a great advance on previous arrangements, we may judge how very inconvenient the latter must have been.

It was now known that the moon's motion was not uniform, and that the planets were still more irregular; in fact, that they periodically retrograded or moved in the opposite direction among the stars, having of course stationary points when the direction of apparent motion changed.

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[1] The accordance is not exact, being about one-fourth of a day in error. Callippus afterwards adopted a period of 76 years, in order to get rid of the error by omitting a day.

[2] In practice they allowed 30 for every month, and omitted every 63rd day; *i.e.* the 3rd day of 3rd month, 6th day of 5th month, etc. It is uncertain which of the 19 years had extra months, but they included the 3rd, 8th, 11th and 19th, with three others.

Eudoxus of Cnidus seems to have been the 370 B.C. first to try to express these motions with the aid of geometry; for each separate motion—the daily rotation; the monthly, annual, or other periodic revolution; the motion in declination (*i.e.* northwards or southwards in relation to the stars); and other irregularities as they came to be noted—were sought to be represented by uniform circular motion, just as modern astronomers represent inequalities by periodic terms. The hypothesis of moving spheres is excellent, in so far as it represented, with more or less fidelity, the observed motions, and it is quite likely that at first this is all that was intended; but the increasing number of spheres became very unintelligible; and, moreover, it began to be assumed that these spheres had a material existence, and carried the various celestial bodies in restricted orbits, thus to a great extent spoiling a useful conception. It must be borne in mind that trigonometry had not yet been invented.

About half a century later—B.C. 300—we come to the earliest extant astronomical works, two books—"Of the Sphere which moves," and "Of the Risings and Settings of the Stars." They are very elementary geometrical treatises, but they mark almost the commencement of geometrical astronomy. The writer, Autolycus, was a contemporary of Euclid, another celebrated geometrician.

We have passed over many names generally associated with the progress of astronomy.

Aristotle, for instance, whose ideas still held sway in the time of Galileo, though now to a great extent discredited, was a younger contemporary of Eudoxus, and really took careful observations of the planets, noting an occultation of Mars by the moon, and of a star in Gemini by Jupiter. But an important school was arising at Alexandria under the first Ptolemy, and contemporary with Autolycus were Aristyllus and Timocharis, whose observations of the relative positions of stars in the zodiac were to form the basis of a great advance in the theory of astronomy. Aristarchus of Samos, who followed them, besides adopting the Pythagorean notions as to the motions of the earth, had a far juster appreciation of its relatively small size than his predecessors; and though, owing to the roughness of his observations, his determinations of the magnitudes and distances of the sun and moon are very inaccurate, yet the theory of his method was sound, and some of his results were, in the circumstances, very good.

Eratosthenes invented the armillary sphere, by which positions of celestial bodies might be referred either to the horizon, the equator, or the ecliptic. The instrument was in great use at Alexandria. With it Eratosthenes determined the obliquity of the ecliptic to be $\frac{11}{83}$ of a circle, or 23° 51′ 19″ 5 in 230 B.C., confirming the secular diminution of the obliquity. With it, also, the equinox could be determined; as, when in the equator the sun would cast the shadow of the equator circle neither above nor below it, and if that time were missed, then at equal intervals, just before and after the equinox, the shadow would be equally above or below, as the case might be.

Eratosthenes also measured the earth, which he believed to be a sphere. At noonday, at the summer solstice, the sun shone down a well at Syene; while at Alexandria, 5000 stadia to the north, the shadow of a vertical stylus made an angle of $\frac{1}{50}$ of a circumference with the stylus itself, hence the circumference of the earth was inferred to be 50 times 5000 stadia. This was probably fairly accurate in the circumstances, but the stadium differed at different times and places, and we do not know what value was employed.

The next astronomer of note, Hipparchus, far transcends all his predecessors and contemporaries in reputation, the effect of his work being comparable with that of Newton in its relative importance to the progress of astronomy in his time. He was born in Bithynia, but observed at Rhodes. He solved the problem, suggested by Plato, to represent the observed motions of sun, moon and planets by uniform circular motions. His hypothesis accounted for the apparent variability of the sun's motion by assuming that the earth was not in the centre of the sun's orbit, so that drawing a line through the earth and the real centre of that orbit, two "apses" are found, at which the sun's distance is respectively least and greatest (perigee and apogee), and where this motion will appear consequently quickest and slowest. Having adopted the hypothesis, Hipparchus proceeded to find the position of the perigee, and the epoch at which the sun was in perigee, and also to determine the eccentricity or distance of the earth from the centre of the sun's orbit. Hence he constructed the first solar tables to give the sun's position among the stars at any time. Such was his genius that he required very few observations in order to determine the sun's orbit. He found the interval from the vernal equinox to the summer tropic or solstice to be 94½ days, and from thence to the autumnal equinox, 92½ days, and from these two periods determined the eccentricity and the apogee with considerable accuracy. The path of the moon is more complicated, being inclined to the ecliptic and intersecting it in points (nodes) not approximately fixed, but obviously moving, and its motion varies in longitude much more than that of the sun, having a ratio of 11 to 13 between its least and greatest values. But from not more than six eclipses, two groups of three, at an interval of 180 years, Hipparchus determined the orbit with sufficient accuracy to represent the greater inequalities which affect the position of the new and full moon, so that eclipses (which only occur at such times) could be predicted from his tables—a severe test, considering the great effect in such calculations of a slight error in the position of the sun or moon.

Then turning his attention to the planets, he had sufficient sagacity to perceive that their inequalities required systematic observations over longer periods for their elucidation. He contented himself with obtaining their mean motions with great accuracy, and then arranged the available observations, adding more of his own, more than doubling the number available, so that some successor after the lapse of years might make effective use of them.

Meanwhile he had partially corrected the value given by Aristarchus for the length of the year. Finding from a comparison of his own observation of a summer solstice with one made by Aristarchus, 145 years before, that 365¼ days was too long, he made it 7 minutes less, and thus still more than 4 minutes too great, but much of this error is probably due to the uncertainty of the observation of Aristarchus.

A new star appearing in the heavens suggested to him the advantage of having a catalogue of the stars visible, so that their configurations should be known and new ones more easily detected. His catalogue contained 1080 stars, but the longitude of some of them, *i.e.*, the angular distance at which they followed the intersection of the equator and ecliptic (the equinox), compared with the longitude found for the same stars by Aristyllus and Timocharis 150 years before, showed an increase of 2 degrees, showing that the equator was slipping back round the ecliptic at the rate of 48 seconds a year. The discovery of the "precession of the equinoxes" marks a notable advance in the science of astronomy, and the value found was within 5 per cent. of the truth.

One other debt we owe to Hipparchus must be mentioned, which is nothing less than the invention of trigonometry, using chords where sines are now used, and also a method of representing the heavens on a plane, from which he deduced the first notion of fixing geographical positions by lines of latitude and longitude. He was also the first to determine longitude by eclipses of the moon, recognising that the eclipse, though visible at several places at the same actual instant, will not be visible at the same local time if the places differ in longitude.

During the next two centuries and a half almost the only notable advance in any branch of astronomy was Julius Cæsar's celebrated reform of the calendar, which was afterwards left practically unaltered till the time of Pope Gregory. Then comes a name of greater, though less deserved, renown than Hipparchus, that of Ptolemy, associated with the Ptolemaic system which for many centuries held sway in the scientific world. The system was not entirely due to Ptolemy, who flourished about 130 A.D., but his great work, generally known as the Almagest (its Arabic title derived from the Greek) was a compendium of all the astronomy known in his time, and hence contained most of the systems of his predecessors. He at any rate adopted and argued in favour of the hypothesis of a fixed earth, being led away by the illusions of the senses which not only objected to a motion that was not felt, but also assumed the earth to be far larger than any of the heavenly bodies, discarding the juster notions of the Pythagorean school. By his system a planet revolves uniformly in a circle (epicycle), whose centre moves uniformly on another circle (deferent), about a point not coincident with the earth (eccentric).

And by suitable adjustment of the proportions of the eccentric, the radii of the two circles and the planet's velocity, he managed to represent with fair accuracy the principal irregularities of the motions of the planets, especially the stationary points and retrogradations.[1]

He determined the parallax of the moon; that is the difference between the direction of the moon as seen from a point on the earth and from the centre of the earth, from which the moon's distance can be found as a multiple of the earth's radius. His method was good but his result, as usual, erroneous. The same fate befell his redetermination of the amount of the precession of the equinoxes, which is in error to an extent five times as great as that of Hipparchus. He published a catalogue of 1022 stars, being that of Hipparchus with a few omissions, but either his observations were very bad, or his tables of reduction erroneous, for the discrepancies in his catalogue are so great that it is confidently asserted that he did not observe the stars at all, or very few of them, but simply brought up the observations of Hipparchus to his own epoch by an erroneous value of precession. It is claimed in support of this view that his tables, though made out for different latitudes, contained no set for that of Alexandria, so that if he observed there he must have employed a troublesome and inaccurate interpolation for the reduction of his observations. On the other hand, it may be that part of the discordances is due to the errors of Hipparchus in the length of the year and other elements. Ptolemy gives so few observations that we have no means of determining the probable error, a course which, if followed nowadays, would lead to the total ignoring of all the results, but there was no one to criticise Ptolemy at the time, and for many centuries afterwards he was still without a rival.[1]

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[1] If an observer stand at night near the middle of a circular track round which another man, while running continually, revolves, holding at arm's length a lighted torch, the torch only being visible, the motion of the light is something like that of a planet seen from the earth, and this is the motion assumed by Ptolemy.

For five centuries the Alexandrian school dragged on, but little was done beyond commentaries on the work of Hipparchus and Ptolemy. Lack of support caused steady decay, and the science was practically dead already in every other place. Then came the Mohammedan conquest, the destruction of the celebrated library by the Caliph Omar, and the final extinction of the science of the Greeks, A.D. 640.

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[1] His other most important work, on geography, is beyond our province.

# CHAPTER IV

## THE ARABS

OMAR himself was a fanatic whose belief that all those books which agreed with the Koran were unnecessary, and that all those which did not ought to be destroyed as heretical, justified him in the destruction of the store-house of learning, which the few scattered fugitives would have no chance of restoring. But some works had escaped the conflagration, and as time went on and the Arabs had leisure from fighting, they began to study and admire, and under the impulse of successive Caliphs the sciences, and astronomy in particular, slowly revived. Almamon, 813 A.D., had all procurable Greek works, including Ptolemy's Almagest, translated into Arabic, the obliquity of the ecliptic determined at Bagdad, and a degree of the meridian also measured. Albategnius, some half century later, was the most celebrated astronomer of the Arabs. He was a Syrian prince, who after studying the Greek methods, began himself to observe, and soon discovered the inaccuracy of Ptolemy's value of the precession. He also determined with still greater accuracy the eccentricity of the orbit of the sun, but through too great faith in Ptolemy, made an error of two minutes in the length of the year. He dis-

## CHAPTER V

### THE REVIVAL—COPERNICUS—TYCHO BRAHE

THE revival in Europe was now beginning to be felt. Already Alphonso, King of Castile, had given an impulse to astronomy by employing the best men he could find on the production of a new set of tables, known as the Alphonsine tables (1488), which, however, were not a success, partly owing to the use of the theory of trepidation already referred to.[1]

The same century had also produced Purbach, who translated the Almagest and had a great reputation as a professor of astronomy, his patron, Cardinal Nicolaus von Cusa, one of the many before the time of Copernicus who suggested that the sun might be the centre of the universe, and his still greater pupil, Müller of Königsberg, known as Regiomontanus, who after many years of study, found a wealthy citizen of Nuremberg, Bernard Walther, to supply him with an observatory and instruments, and to share the labour of making observations, which Walther continued for thirty years after Müller's death; introducing in 1484 the use of clocks in astronomical observations. But a great revolution was just approaching. For many centuries the Ptolemaic system had continued unchallenged. It sufficed to explain with fair accuracy the apparent motions of the celestial bodies; and it made no great demands against the evidence of the senses. It would have fallen at once if any modern means of taking accurate observations had existed, for though it represented the direction and velocity of the various motions, it would have failed entirely when applied to the varying distances from the earth. Hipparchus had endeavoured to determine these with a special instrument, and had he succeeded, the history of astronomy might have been very different.

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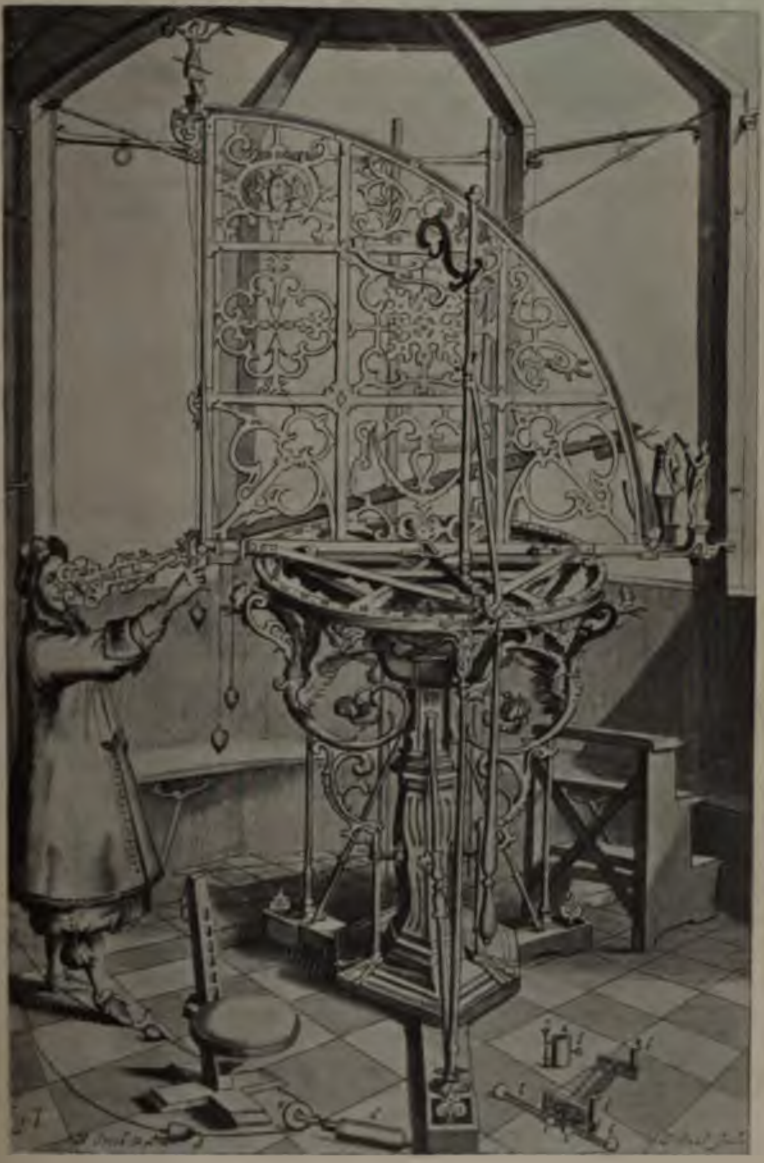
[1] It was he who in disgust at the complexity of the Ptolemaic system, expressed his regret that he had not been consulted at the creation of the universe.

It remained for Copernicus to inaugurate a new era. This great man was born at Thorn, in Polish Prussia, in 1473, and after studying medicine, found himself so strongly attracted to mathematics, that, having an uncle who was a bishop, he took orders with the view of having enough to live upon while pursuing his scientific studies. He saw that the daily rotation could be explained just as completely by the rotation of the earth itself from west to east, as by that of the whole universe from east to west. But having to choose between the simplicity of the one, which Ptolemy had rejected as contrary to common sense, and the obviousness of the other, which Ptolemy had deliberately adopted in spite of the enormous motions it involved, Copernicus came to the opposite conclusion, and set to work to prove it the true one by demolishing the objections to it. He pointed out that only relative motion is perceived by the senses, and that the air was bound to share the motion of the earth, so that the terrible winds suggested by the Ptolemaists as a necessary consequence of the earth's rotation would have no existence. Having thus abolished the obviousness on which Ptolemy relied, the simplicity of the opposite system was bound to prevail. Copernicus also saw the extreme improbability of the real existence of Ptolemy's sphere of the stars, which necessitated the assumption that they were all at the same distance from the earth, which, though not quite impossible, was in the last degree improbable. He also from a juster appreciation of the enormous distance of the stars deduced that they must be of vast size and not mere points, so that the assumption of their daily revolution about the earth became still more preposterous. The next step was less simple, as it was by no means obvious that the motion of the planets round the sun would produce the stationary points and retrogradations to account for which Ptolemy's complicated system of epicycles was invented. But having once admitted that the earth moved in one way, it was less unlikely that it moved in other ways, and by diligent application Copernicus evolved his system by which all the planets, including the earth, revolved about the sun. His theory was far from perfect, for he still adhered to circular motion, so that as the motion referred to the sun was certainly not circular, he had to assume a centre for each planetary orbit outside the sun, and all different, each involving an epicycle as under the Ptolemaic system. He also supposed a third motion of the earth to account for the axis always being in the same direction, being misled by the distance of the point to which the axis is directed; this distance is practically infinite, so that the earth's axis does not need any motion in order to keep pointing to it as it would if the point were near, as in any orrery or mechanical representation.

He foresaw the storm of opposition his new theory was bound to encounter, and for a long time (he himself says thirty-six years) refused to publish it. At length, in 1543, his "De Revolutionibus Orbium Cœlestium," was printed, but the only copy he ever saw was brought to him on his death-bed in 1543 and never opened by him. His fame is perhaps greater than he deserved, for his theory did not claim originality, and it certainly lacked completeness. He probably owes much in that respect to the reaction after so many centuries of thraldom to a system so highly venerated as Ptolemy's.

The new system soon obtained a hearing after the death of its author, and was adopted by some of his pupils before publication, but it also aroused vehement opposition, not so much from astronomers as from the Church of Rome, though even among learned men it was by no means generally accepted. Bacon, for instance, never fully assented to it; and it was only after its confirmation by facts, which was a natural consequence of the invention of the telescope, that it became firmly established. Before that time arrived, however, astronomy received valuable assistance in a totally different direction.

Copernicus was a theorist; his observations were of no great value. In Tycho Brahe we find just the reverse. Born of a noble Danish family three years after the death of Copernicus, he was first attracted towards astronomy by a solar eclipse partially visible at Copenhagen in 1560, the year after he had been sent to the university there. With such tables as he could obtain he set to work to make observations, and being sent to Leipsic to study law with a tutor, continued his night work with a small globe and a pair of compasses. Even with such elementary means he soon discovered that not only the Alphonsine tables were far from correct for the places of the planets, but that even the more modern Prutenic tables, computed by a follower of Copernicus, were several days in error. He gradually obtained better instruments, and made the important advance of determining the errors of his instruments and making tables of corrections. In his visits to Denmark he found very little but contempt for liberal knowledge, and returned again and again to Germany. In 1569 he went to Augsburg, and had a large wooden quadrant constructed, with which he made regular observations, until he once more returned home in 1571, and his maternal uncle, the only relative who had encouraged him at all, offered him part of his house, providing an observatory and a laboratory, for he had lately begun working at chemistry also. Tycho here constructed a large sextant, which he used assiduously; and here, on November 11th, 1572, he saw for the first time the new star,



AN OLD QUADRANT (HEVELIUS)

COPERNICUS

TYCHO BRAHE (1546-1601)

always associated with his name, a bright object in the constellation of Cassiopeia, which became as bright as Venus and visible in the daytime, and then faded gradually until March 1574, when it was no longer within reach of the unaided eye.[1]

He at once began a series of observations to determine if possible the parallax of the new star, in order to discover whether it could be anything but a star, and these observations are the basis of his first published work, published at the earnest desire of friends at Copenhagen University, as he himself considered it beneath the dignity of a nobleman.

Facilis descensus Averno! He next married a plebeian girl on his own account, and then was almost compelled, by the intervention of the King, to deliver lectures in order that others might profit by his learning, discoursing in defence of astrology and on the Prutenic tables.

He then travelled in Germany and Switzerland, seeking a location for an observatory, but first made a friend of a persevering observer, the Landgrave of Hesse Cassel, who strongly recommended the King of Denmark to recall Tycho and become his patron. Accordingly the King (Frederick II.) offered him the island of Hveen in the Sound, with certain revenues and a pension, and guaranteed the expense of building an observatory. Tycho Brahe gladly accepted the offer, and Uraniborg was built, and equipped with the best instruments obtainable.

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[1] It has been claimed that a small telescopic star is still to be found in the same place.

In 1577 he began work, and observed, among other objects, a bright comet, from the motion of which, in conjunction with those of other comets (he observed seven in all), he was enabled to point out that the spheres of the planets could not be solid, as comets passed through freely in every direction, and also that the comets, so far from being atmospheric phenomena, were much further off than the moon. For about twenty years the work went on, a students' observatory being excavated in the grounds of Uraniborg, principally for the use of Tycho's disciples. Tycho had a very strong belief in his own powers and in the value of his work, and the glory that ought to accrue to Denmark therefrom, and as he was naturally not accepted quite at his own valuation, friction necessarily arose from time to time, on the one hand between him and his tenants, and on the other between him and the Court party, whose influence, especially after the death of the King, added to his own want of tact, gradually brought about such a diminution of the revenues originally granted for the endowment of Uraniborg that ultimately the place was abandoned. Some of the instruments were temporarily used at Copenhagen, until Tycho withdrew once more to Germany, and after some unsuccessful attempts to justify his action and regain favour with the young King Christian, obtained a new patron in the Emperor Rudolph II., who received him at Prague, and granted him a fine house and a pension, which, however, he did not live long to enjoy.

His observations, with the means he was able to employ (before the invention of the telescope), are superior in accuracy to any previously made. He was able to compute the first table of refractions to an altitude of 45°, beyond which the refraction, though very sensible with a telescope, was probably too small for detection by his instruments. He discovered, or more probably (*v. sup.*) rediscovered P. 26. independently, the lunar inequality known as the "variation," by observing the moon at all phases instead of only at the quarters, to which his predecessors had generally confined themselves, the oldest observers having noted nothing but eclipses, which could only give the mean motions. When the earth and moon are equidistant from the sun, *i.e.* in quadrature, the attraction of the sun tends slightly to draw the two together and hence to increase the earth's pull on the moon and so accelerate the moon. At new and full moon, however, the effect is just as in the ocean tides, to draw the new moon away from the earth or the earth away from the full moon, and so retard the moon. Thus the moon's velocity would, as far as this cause affects it, be a maximum at first and last quarter and a minimum at new and full moon, so that through each quarter, though the week's motion would be nearly the same on the whole, yet it would necessarily be accelerating and retarding through alternate weeks, and halfway between the quarters would be furthest from where it would be if this inequality did not exist.

In these cases, as Tycho discovered, it could be more than its diameter away from its previously calculated place. This "variation," however, depends also on the distance of the sun, and as this is less in winter, the moon moves on the whole more slowly in longitude in the winter, and thus an annual inequality is caused, which Tycho also noted, but which is only about one-third as great as the variation. He also discovered the varying inclination of the moon's orbit to the ecliptic, so that he left the lunar theory considerably more advanced than he found it. He also determined the positions of 777 stars with great accuracy, his catalogue having thus immense superiority over those of Hipparchus and Ulugh Begh; though, unfortunately, in order not to appear inferior even in numbers to that of Ptolemy, he inserted more than 200 positions of stars hastily observed in order to bring the number up to a thousand.

His most important incursion into the region of theory, his planetary system, has not perhaps met with even the credit due to it. Copernicus, as we have seen, faced with the difficulty of the enormous motions of the stars on the one hand, and the apparent immobility of the earth, backed by centuries of dogma, on the other, rejected the latter as the lesser difficulty. Tycho Brahe did not; though he adopted the simplification of making the planets revolve round the sun, yet he supposed the whole universe to turn about the earth. He considered, with very good reason, that if the earth really moved round the sun, the stars would show it by apparent displacement, and as the stars to him, being, as must be always remembered, without telescopic aid, seemed to be large, he had a different dilemma to face, of which Copernicus, not being an assiduous observer, had probably no idea, as in order to appear so large and yet be so distant as to show no annual displacement, or parallax as it is called, the size of the stars must have been inconceivably great. Moreover it was urged that if the earth rotated, a stone falling from a high tower would deviate from the vertical. Copernicus supposed that the air carried it, to evade the difficulty. Tycho apparently denied the rotation, but it must be remembered that he was hardly free to criticise the views adopted by the Church and his patrons. The only difference between the two systems being that in the one case the sun was fixed, and in the other the earth, all the other motions being mathematically the same, there were good grounds *at the time* for preferring Tycho's system, for it was only after his death that the theory of mechanics cleared away one difficulty, and the invention of the telescope, by proving that the stars are at such a distance as to appear only as points instead of large discs, removed another, while the researches of mighty thinkers so elaborated the crude form in which Copernicus left his system that very little more than the name remains unmodified. It is hardly fair, therefore, to compare the modern form of the Copernican system with the unaltered one of Tycho. Besides, by his cometary researches, which demolished the "solid" spheres of Ptolemy, he did yeoman service to all Ptolemy's opponents, including Copernicus, and by the long series of careful planetary observations which he saw to be necessary to support his own or any other system, he provided the materials for the next great advance, which is due almost as much to him as to Kepler himself.

# CHAPTER VI

## KEPLER—GALILEO

JOHANN KEPLER, the real founder of modern astronomy, after instruction under Mæstlin, one of the first teachers to adopt the opinions of Copernicus, secured the appointment of " mathematician " at Gratz in Styria, and obtained the notice of Tycho Brahe by dissertations on celestial orbits. His position at Gratz becoming untenable on account of his religious opinions, which were of the school of the reformers, he ultimately came to join Tycho at Prague, and though a Copernican, in spite of the Dane's arguments in favour of his own system, was entrusted with the reduction of Tycho's observations of Mars, and soon afterwards came into practical possession of the whole collection of observations by the owner's death. He was obviously the man to be entrusted by the Emperor with the preparation of the Rudolphine tables founded on Tycho's observations. For twenty years he laboured on them, at first under the ancient fixed idea of circular motion, which, however, he was finally compelled to abandon in favour of a new law, henceforward to be known as Kepler's first law of motion, to the effect that every planet moves in an ellipse of which the sun occupies one focus. As it was soon obvious that this motion was not uniform, as the ancients understood the term, he was led to the discovery of his second law, that the area swept out by the radius vector from the planet to the sun was proportional to the time. It seems a very fortunate circumstance that he should have set to work first on Mars, as, of all the planets then known, Mars has the orbit most widely differing from a circle, so that he may be said to have started on the most favourable case. How much longer it would have taken him if he had started with Jupiter, for instance, or whether he would have succeeded at all, is, of course, idle speculation. We can only rejoice that at least that difficulty was spared him. So far the work, though long, was fairly straightforward. He computed seven oppositions of Mars before abandoning the idea of circular motion ; but he desired, more than all, to find some relation between the motions of different planets, and for years he sought it vainly, until it occurred to him to try different powers of the times and distances, and then he arrived at his third law, that the square of the periodic time of any planet is proportional to the cube of the mean distance. This, as well as his other laws, he verified for all known planets and for the moon, and they have since been proved to hold for all planets and satellites. The first result of these brilliant discoveries was to ensure for the Rudolphine tables an accuracy far exceeding that of any previous ones. He did not apply his theory to comets, being imbued with a preconceived notion that they never returned, inasmuch as, according to

KEPLER (1571-1630)

GALILEO (1564-1642)

his idea, the tail of the comet was evidence that the sun was driving the body of the comet, particle by particle, away, and thus dissipating its substance for ever. He pointed out the great utility of eclipses for determining differences of longitude, and the extension of this method to occultations of stars by the moon is still in use. In searching for the physical causes underlying his laws, he came very near the truth, for he discovered that attraction between two bodies was mutual and proportional to the mass, and varied with the distance, but the law of variation escaped him, as he assumed it to be simply as the inverse distance instead of the inverse square of the distance. He suffered all through his career from want of money. His stipend was very irregularly paid, and he lost much time in trying to collect it. The strain on his mind and the continual journeys which this unfortunate state of affairs involved contributed to shorten his life. He died in 1631, in his sixtieth year.

Contemporary with him and of undying fame in yet another branch of astronomy was Galileo Galilei, more commonly known by his first name. Born at Florence in 1564, and educated at Venice and Pisa, he became a professor at Padua, after a brief and meagre appointment at Pisa, until induced by Cosmo de' Medici (Cosmo II.) to remove to Florence. His discoveries in mechanics were of far-reaching importance. By experiments carried out at the celebrated leaning tower of Pisa, he established the law of acceleration of falling bodies, and by observation of a swinging lamp during some long cathedral sermon he was led to the principle of the isochronism of the vibrations of a common pendulum—*i.e.* that a pendulum of a given length has its time of swing independent of the extent of swing, a principle which rendered immense service to astronomy by making accurate clock-driving possible. But he is chiefly celebrated for reasons of a totally different kind. The combination of a convex and a concave lens to magnify distant objects had been accidentally discovered in Holland, and Galileo heard of it, and at once set to work to try his hand with lenses in a leaden tube. He soon constructed a telescope which magnified 32 times, and turning it towards the moon, discovered the irregularity of its surface. Turning next to the planets, he discovered that Venus exhibited phases similar to those of the moon (just as Copernicus had predicted would be the case, according to his system by which Venus revolved about the sun), that Jupiter was accompanied by four moons, moving round the great planet just as our moon does round the earth, which, in compliment to Cosmo, he named the "Medicean stars," and that Saturn at times appeared like three stars joined together and at other times only one. He also was the first to record spots on the sun, and to deduce its time of rotation from their motion. These discoveries, marvellous as they must have appeared, were, of course, a necessary consequence of the invention of the telescope, which was not due to Galileo, although his particular form of telescope was worked out by himself. The discoveries themselves were claimed for other observers or denied altogether, and, at the time, the principal result of his powerful analysis and deduction from his observations, which should have gone far to destroy the authority of Ptolemy and Aristotle, was to raise up such a storm of opposition from the schoolmen, and especially from the Church of Rome, that he was obliged first to publish his system as a mere hypothesis, and subsequently to abjure much of it, including the motion of the earth. He was the first to suggest eclipses of Jupiter's satellites as a means for determining differences of longitude, the difference between the local time at two different stations of an instantaneous phenomenon visible at both being, of course, the difference of longitude. He also discovered the "libration" of the moon, by which, according to its position in its orbit, we sometimes see a little way round the corner, so to speak, in different parts of the moon's apparent edge. The sentences passed against him by the Inquisition on account of his so-called heretical opinions as to the motion of the earth and the similarity to it of the other planets, which Aristotle had maintained to be divine and incorruptible essences, were not strictly enforced. He was imprisoned, but his patron procured his release on condition of perpetual exile from Florence. He unfortunately became blind before his tables of Jupiter's satellites were completed, and died a few years later, in 1642.

Meanwhile a great invention saw the light in

Scotland in 1614, when Lord Napier of Merchiston invented logarithms, in reference to which Laplace says: "An admirable artifice which, by reducing to a few days the labour of many months, doubles the life of the astronomer, and spares him the errors and disgust inseparable from long calculations." It is indeed difficult to conceive how accurate astronomy could possibly have advanced with any speed without this invention.

But we must pass over many well-known names with the barest reference. Bayer introduced the method of naming stars by means of the Greek alphabet in the different constellations. Scheiner made series of observations of sun-spots; Horrox and his friend Crabtree were the first to observe a transit of Venus, in 1639, and they would probably have done good work for astronomy had not both died very young. Gassendi observed a transit of Mercury in 1631. Riccioli did good service as a collector and publisher of the work of others, assisted by Grimaldi, who mapped the moon and gave many of the names, now familiar to astronomers, to its principal features. The last and probably the most accurate of observers without telescopic sights was Hevelius, of Dantzig (1611-1687), celebrated for his careful drawings of different phases of the moon, and for the tenacity with which he clung to his old instruments, refusing to adopt the improved telescopic method, which was urged upon him by Halley during a visit, and thus greatly impairing the value of his observations.

Huyghens (1629-1695) first adapted the pendulum

AN OLD MAP OF THE MOON

to clocks, and improved telescopes so much that he was able to discover the fact that the curious appearance noticed by Galileo in regard to Saturn, was caused by a flat bright band surrounding the planet, and known as Saturn's ring. The adoption of the telescope in conjunction with the quadrant was, after more or less tentative use by others, definitely inaugurated by Picard in 1667, and about the same time micrometers of different sorts were invented by Gascoigne and others. Picard introduced the method of determining the right ascensions of stars by observation on the meridian, a method soon greatly improved in accuracy by his pupil Roemer, who invented the transit-instrument, and is also famous as the discoverer, from the apparent irregularity of the eclipses of Jupiter's satellites, that light does not travel instantaneously. Science, at the same epoch, received a great impetus in many ways, the short space of fifteen years seeing the foundation of the Observatories of Paris and Greenwich, and of the English Royal Society, and the French Academy of Sciences, the Italian Academy of the Lynx-eyed (dei Lincei) being half a century older.

Dominic Cassini, the first director of the Paris Observatory, though not connected with any of the epoch-making improvements of his time, made several interesting observations. He discovered the well-known division in Saturn's ring that bears his name, and four satellites of that planet, in addition to one discovered by Huyghens. He measured the rotation of Jupiter and Mars; constructed very accurate tables of Jupiter's satellites, and the first calculated tables of refraction; observed the zodiacal light, and made a near approximation to the solar parallax; while he also produced a complete theory of the moon's libration. The real revolution in astronomical science, however, as Delambre points out, was wrought not by observations like these, but by the "heresy" of Copernicus, the laws of Kepler, the pendulum, the micrometer, the sector and mural circle, and the transit-instrument.

These heralded the rising of the immortal genius of Newton, the rapid improvement in the accuracy of observations paving the way for the testing of the great theory, which without them might have languished in obscurity, if not in discredit.

# CHAPTER VII

## NEWTON

ISAAC NEWTON, born on Christmas Day 1642, the day of Galileo's death, was in Cambridge at the time of the Great Plague, which drove him, among many others, to retire to the country for a while. Meditating upon the central force that keeps the planets in their orbits, it occurred to him that a force similar to that which causes a body to fall to the earth, might, by continually deflecting the moon from a natural straight path, constrain it to revolve about the earth. Having already proved from Kepler's laws that if planets described circular orbits about the sun, the force towards the sun would vary inversely as the squares of the distances, he computed the distance through which the moon is deflected in one minute from the tangent to her orbit, and found it 13 feet. Then reasoning from the distance a body falls in a second at the earth's surface, and applying his law of the inverse square, he found that from this cause the moon would be deflected 15 feet, and this discrepancy led him to lay aside his brilliant conjecture. Several years later his mind was again directed to the same question, and in the interim a far more accurate measurement of a degree of latitude had been made by

Picard. This correction to the size of the earth completely removed the discrepancy he had found before, and established the truth of his hypothesis.[1] But the law of the inverse square was still unaccepted; in fact, Newton had not apparently published much, if any, of the results of his researches. In 1684 Wren, Halley and Hooke discussed the problem of motion in an orbit under a central force varying as the inverse square of the distance; Hooke claimed to have solved it, but as he did not give his solution Halley applied to Newton, who told him he had already solved it, and that the result was an ellipse. By Halley's instrumentality he was induced to send in a memoir to the Royal Society, containing some of the propositions afterwards embodied in his "Principia." Meanwhile Flamsteed, the first Astronomer Royal, was working away at Greenwich with a very moderate equipment, to carry out the founder's object of so improving by accurate and continued observation the computed places of the moon and planets and brighter stars, as to enable them to be used with confidence in navigation. To him Newton naturally appealed for more accurate places and elements for planetary orbits, as he was still testing the accuracy of Kepler's numerical third law, in order to apply his new theory to every planet and satellite known in the heavens; and though, probably because Flamsteed was only an observer and not a theorist, he did not respond with sufficient enthusiasm to Newton's

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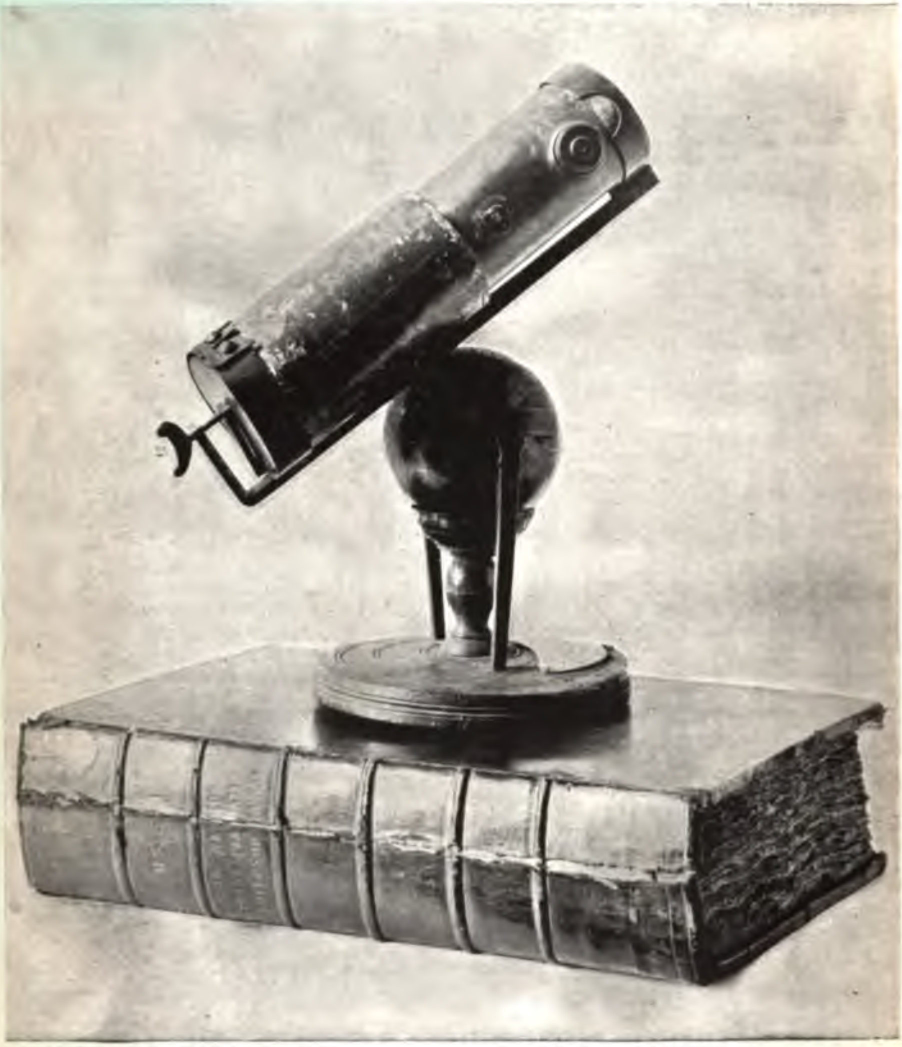
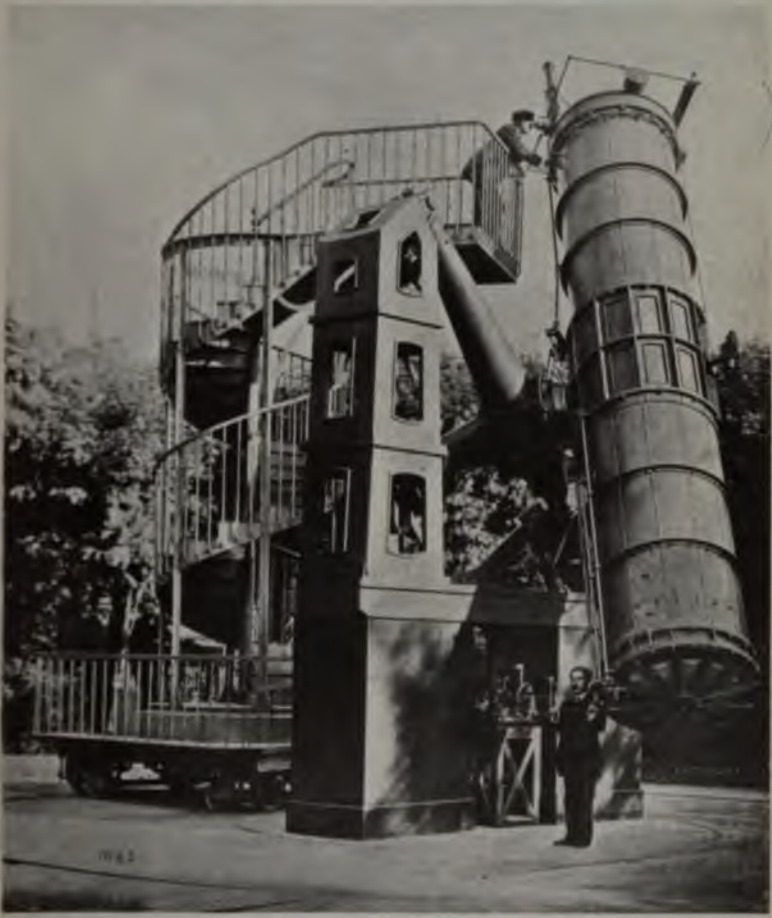
[1] There are strong grounds for doubting the accuracy of this, the generally accepted explanation of the delay.



NEWTON'S REFLECTOR



PARIS 4-FOOT REFLECTOR

repeated requests, the results he did send were of great value for the purpose just indicated. To prove the universality of Newton's law of gravitation, as it is called, by any direct argument is a practical impossibility. All that could be done was to apply it to case after case in the hope that every single one would conform to it, and the "particular negative," the only thing required to disprove the "universal affirmative," fail to materialise. This hope has not yet been destroyed, though we shall find suggestions as to the possible identity of the "particular negative," in the guise not of an exception to the universality of the law, but of a suspicion as to its accuracy. But besides the law of gravitation, in itself a monumental achievement, we owe to Newton's mathematical talent a great mass of theorems which, though many of them in their original form, owing to his unwieldy method of fluxions, long remained too abstruse for students, have ultimately tended to lighten the labours of mathematical analysis. In optics, moreover, his labours were crowned with a great measure of success. His failure to hit upon any device to get rid of chromatic aberration in object-glasses led him to invent and construct the Newtonian form of reflecting telescope. His investigation of the refraction of light through a prism helped to lay the foundation of the spectroscopy that plays such a conspicuous part in modern astronomy.

We must not dismiss the law of gravitation without studying some of its consequences with a view to gaining a juster appreciation of its importance. Newton's law translated into English runs thus: "Every particle of matter in the universe attracts every other particle with a force varying inversely as the square of their mutual distances, and directly as the mass of the attracting particle." He had previously established the theorem that any body composed of concentric spherical shells of different density attracts as if all its mass is concentrated at its centre. It thus became possible to regard the celestial bodies as points. He proved that a body projected in space under a central force according to his law must describe a conic section, either a parabola, or an ellipse, or a hyperbola, or a circle, and that knowing the initial distance of the body from the seat of the central force, and the direction and velocity of the initial motion, the orbit could be completely determined. From this he found that comets also obeyed the law. He realised, however, that the presence of other bodies exercises a disturbing influence on orbital motion (*i.e.* causes perturbations).

Regarding the moon as moving about the earth, but perturbed by the attraction of the sun, he demonstrated that her apsides will advance and her nodes regress,[1] with reference to her orbit, facts already established by observation. He proceeded to compute many of the important lunar inequalities. He discovered that the mutual attraction of the particles of the earth, combined

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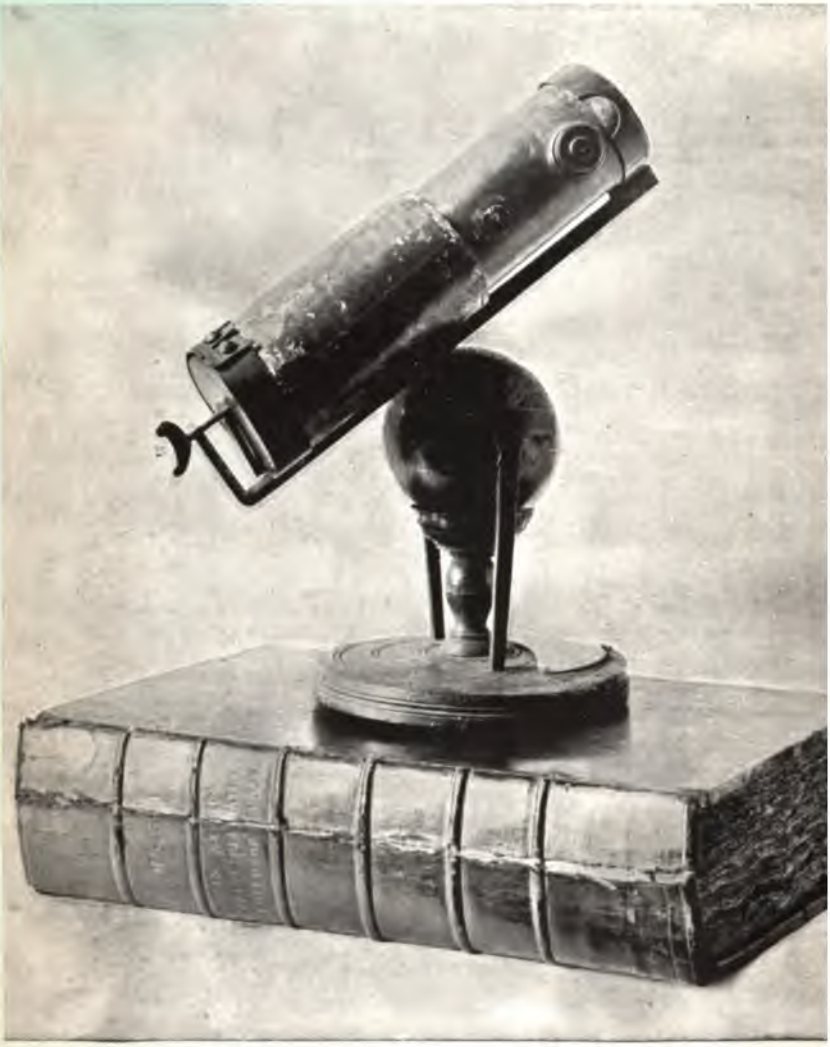
[1] The apses, or the apogee and perigee, are the extremities of the major axis of the undisturbed relative orbit. The lunar nodes are the points where the lunar orbit intersects the plane of the earth's orbit.



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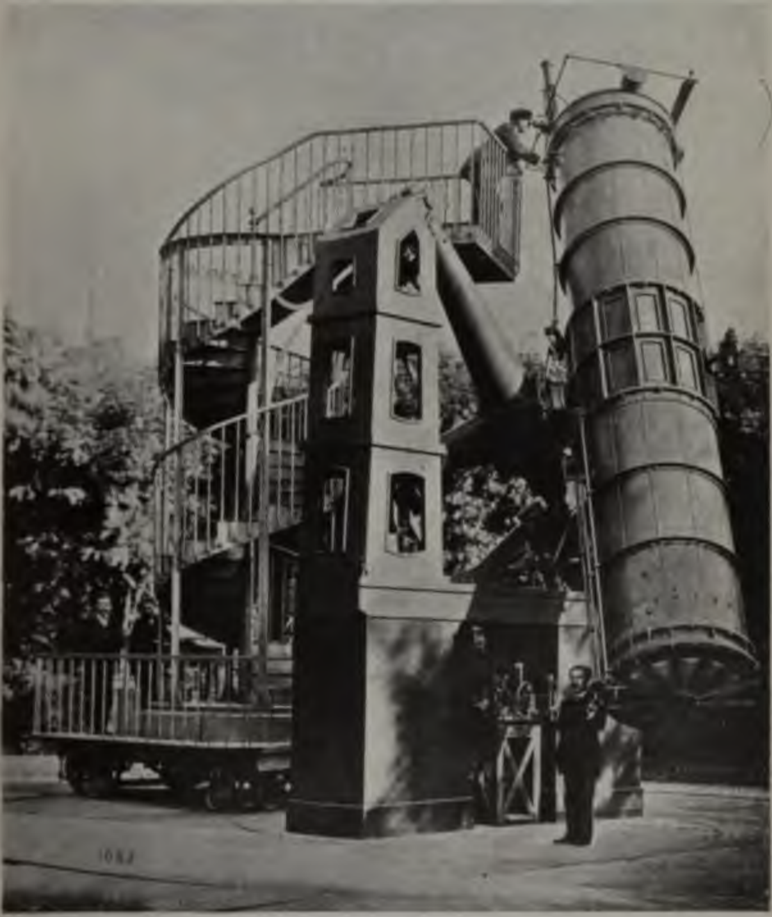

PARIS 4-FOOT REFLECTOR

the geometers of the last century, all presented with much elegance, will assure to the ‘Principia’ a pre-eminence above all the other productions of the human intellect.”

It is quite possible that in this connection we owe a greater debt to Halley than is generally realised. Halley attacked the problem of elliptic motion without success, starting from the principle of the inverse square. Failing to get any result from his own work or any real assistance from Hooke, who claimed to have solved the problem, he singled out Newton as the man for the investigation, and, finding it already disposed of, induced him to take it up again and elaborate it. He next persuaded him to send his book to the Royal Society, and convinced that body that it ought to be published; but even then, owing to the Society's want of funds, the publication hung fire until Halley himself paid the expenses of it.

# CHAPTER VIII

## NEWTON'S SUCCESSORS: LAPLACE

EXCEPT in his own country, Newton's theory was received with great hostility. On the continent, where Descartes' theory of vortices, though only a hypothesis, and quite inapplicable to comets, had received much support, it was long before Newton's work received much recognition. Huygens, Leibnitz, Cassini, and others, though some of them admitted the truth of some part of the hypothesis, each from his own point of view opposed the system as a whole. But after half a century Voltaire published a short but clear and popular treatise on Newton's principal discoveries in optics and astronomy, and from that date, 1738, Newton's principles may be said to have held the field. During the intervening period Newton's followers made no real advance, his method being so difficult that it is said that in that direction no further step, with one possible exception,[1] has ever been made. Nevertheless his opponents were unconsciously working for him, by improving and perfecting the new calculus, which, after its invention by Newton and Leibnitz, made great progress under the latter and the Bernouillis, and in course of time provided an instrument whereby lesser intellects might continue Newton's investigations.

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[1] M'Laurin's "Investigation of the attraction of an Ellipsoid."

In one conspicuous instance Newton's theory had appeared to fail, his determination of the motion of the moon's perigee being just half the observed quantity, and this point was soon to attract attention, when work in this direction was once more undertaken.

Euler, Clairaut, and D'Alembert were the first geometers to proceed beyond the point reached by Newton, and all three independently sent memoirs to the Academy of Sciences at Paris in 1747, the prize for 1748 having been offered for an investigation of the inequalities of Jupiter and Saturn. The subject of perturbed motion is one of great difficulty, but in the problems presented to the astronomical analyst, it happens that it can in nearly every case be simplified. Instead of considering the motion of a planet under several forces directed to all the other bodies of the system we may regard the sun, being of enormously greater mass than any of the planets, as responsible for the controlling force and each separate inter-planetary influence as a perturbation so small that its effects may be considered separately. Hence the problem reduces to that of "three bodies," which also in general is beyond solution, but in the particular cases we have to consider is simplified by the relatively small effect of one of the three. Thus in the lunar theory, the sun's great distance compared with that of the earth renders his effect on the moon's relative orbit so small in spite of his enormous mass, that it can be treated as a perturbation, though in this case the perturbations are much larger than in that of the planetary orbits. For this reason the lunar theory was rightly regarded as a favourable case for testing the law of gravitation, and the three geometers all arrived at the result already obtained by Newton, that the theoretical value of the motion of the moon's apogee was only half the observed quantity. Was the law going to break down? Clairaut first suggested a modification of the law, another term varying with the inverse fourth power of the distance being introduced, but at length all three, revising their work and taking into account some small terms previously neglected as unimportant, arrived at a new value just double of that previously obtained, and gave confirmation of Newton's law, the stronger in proportion to the seeming failure thus overcome. The lunar tables published by these three geometers as the result of their investigations were of varying accuracy. D'Alembert, for instance, relied too much on theory, and though Euler revised and re-revised his, yet they were in the end inferior to those of Mayer, who adopted Euler's theory but skilfully combined it with numerous observations. The English Board of Longitude, which had offered a prize in connection with any method of obtaining the longitude at sea, appointed Bradley to adjudicate on these tables, to determine whether the method of lunar distances computed by their aid would give the desired accuracy, and on his report that no error exceeded a minute and a quarter of arc, Mayer's widow was awarded £3000.

But as before remarked, it was not the lunar theory but that of Jupiter and Saturn which had been proposed by the Academy of Sciences for the prize in 1748. Euler's memoir, though successful, did not account for the great inequality which required explanation. After the confirmation of the Newtonian theory by Clairaut in connection with the motion of the moon's apogee, the same question was proposed by the Academy in 1752, and again Euler was successful in gaining the prize, though failing as before to reconcile the observed inequality with theory. He had, however, by this time hit upon the germ of many successful investigations by practical application of the theory of the variation of arbitrary constants. For a planet moving in an elliptic orbit we require six constants to fix (1) the size, (2) the shape, (3) the position of the orbit in its own plane, (4) the inclination and (5) intersection of the orbit with a fixed plane, and (6) the position of the planet in the orbit.[1]

The theory of Euler, further developed in connection with a memoir on the perturbation of the earth, crowned by the Academy in 1756, was that the motion of a planet could be regarded at any moment as performed in an ellipse whose constants were continually changing under the action of other planets, the effects of which may be studied separately on each of the six elements. With his fertility of invention and command of analysis, it is a pity that grave errors of calculation prevented his attaining the success due to his ingenious methods. We owe to him many other analytical results, extending from simple trigonometry to differential equations, but his greatest service to theoretical astronomy was that indicated above, known as the method of the variation of the arbitrary constants. His own work in the theory of perturbations, though crowned again and again by the Academy of Sciences, continually fell short of absolute success, but along those lines his immediate successors were enabled to attain a high degree of perfection in the same theory. Euler himself in a memoir on the motion of Jupiter and Saturn had arrived at the result that the inequalities arising from the mutual action of the two planets ultimately compensated each other after a very long period; Laplace, omitting the higher terms in the expressions for eccentricity and inclination and those depending on the squares of the disturbing masses, obtained the further advance that the mean distance of every planet, and consequently the mean motion, is invariable. Lagrange in 1776 completed the 1736-1813. result, including all the terms of the expressions, and established the important principle that all the planetary perturbations are periodic, thus proving the stability of the solar system.

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[1] (1) The mean distance, (2) the eccentricity, (3) longitude of perihelion, (4) inclination, (5) longitude of node, (6) epoch.

For a considerable period the history of theoretical astronomy consists of a succession of triumphs of analysis, first Lagrange and then Laplace taking a fresh step in advance towards the completion of the great fabric of theory arising directly from Newton's discovery. Lagrange did much towards perfecting the calculus of variations in succession to the work of Euler, thus providing an instrument wherewith his own successors might the more easily attack fresh problems in the same field. He worked out a complete and rigorous solution of the problem of the moon's libration in 1780, after a partial success in 1764, and succeeded admirably with a modified problem of six bodies in 1766, when in a memoir on the theory of Jupiter's satellites, he included the sun as a disturbing body. His great contemporary Laplace having attained success at the 1749-1827. early age of twenty-three in the restricted proof of the invariability of the mean motions of the planets, became a member of the Academy of Sciences, and devoted himself to the series of memoirs which formed the foundation of his famous "Mécanique Céleste." It had for some time been suspected, from the comparison of ancient and modern eclipse observations, that the moon's mean motion had been slowly accelerating. Dunthorne and Mayer, confirming Halley's suspicion, computed the amount of acceleration at 10″ in a century. Lagrange having proved that this could not be due to the figure of the earth, two hypotheses were advanced—one the action of a resisting medium, and the other a finite velocity for the action of gravitation. The latter supposition was at first assumed by Laplace, but on finding that the eccentricity of Jupiter's orbit affected the mean motions of his satellites, he at once transferred this idea to the motion of the moon, and by proving the theoretical effect of the eccentricity of the earth's orbit to be just such an acceleration as had been observed in the moon's mean motion, he overthrew what was at that time the last barrier to the universal application of Newton's law as the physical explanation of all celestial motions. His next great success was in the direction of the cause of the long inequalities in the motion of Jupiter and Saturn. The general principle involved is the case of the commensurability of mean motions. It occurred to Laplace that as five times the mean motion of Saturn is nearly twice that of Jupiter, terms in the differential equations of motion involving such an argument as (5S - 2J), although multiplied by very small factors, might become very important, and, after a laborious analysis, proved that this was the case, and that Saturn had a long inequality of a period of 929 years, and a maximum value of about 48′, the corresponding value in Jupiter's case being nearly 20′ in the opposite direction, but that these were also subject to the secular variation of the elements. He investigated the figure of the earth in a general way from two lunar inequalities depending upon the ellipsoidal form of the earth, and computed the theoretical ellipticity of the meridian section to be about $\frac{1}{305}$. He greatly advanced the theory of tides, taking into consideration the earth's rotation, which had been previously ignored in this connection, and obtained a complete set of differential equations, from which he deduced that the depth of water is an important factor, and that if this were uniform, some irregularities would vanish, that the fluidity of the sea does not affect the motion of the earth's axis, and that the equilibrium of the ocean is stable, and that it cannot, therefore, of itself alter its distribution over the surface. In the same connection, having proved as just stated that the tides do not affect precession and nutation, he made a further investigation of this subject, and showed that the annual variation of precession causes a variation in the length of the tropical year, which is about 10 seconds shorter than it was 2000 years ago. Many other labours in astronomy, to say nothing of mathematics and physics, the chief of which may, perhaps, be considered the complete theory of Jupiter's satellites, which was the foundation of Delambre's tables, render Laplace's place second only to that of Newton among the benefactors of these sciences. Between him and Lagrange, up to the latter's death, a friendly rivalry was maintained, though at times there might have been suggestions as to appropriation of ideas; but Laplace had the great advantage of a better balanced mind apart from his scientific work, and though his short political career as Napoleon's Minister of the Interior was a conspicuous failure, he succeeded in carrying out the principles of the Vicar of Bray to such an extent that he obtained a marquisate and other honours with scarcely a single set-back. The nebular hypothesis, by which alone he is known to many people, was considered by him so speculative that it appeared only in a note, in his "Système du Monde." It has been the model of many subsequent speculations, not perhaps on account of its real probability so much as for its simplicity and clearness. Briefly it suggests that the solar system was originally a nebula extending at least as far from the sun as the furthest member of the system; that this nebula was rotating and condensing, and that the high velocity induced in the outer portion, as the rotation velocity increased with the shrinkage by a well-known physical law, from time to time caused the outermost portion to break off in the form of a ring still revolving about the centre; that some of these rings, by unequal condensation, gradually condensed into planets, with or without satellites, by a further application of the same principle, one of them, Saturn, having still a set of rings, hitherto uncondensed, as rudimentary satellites, and one of the original rings, by more uniform condensation, having condensed into a large number of separate bodies, the minor planets, instead of into one system. The hypothesis has the advantage of giving an easy explanation for the rotation and revolution of all the members of the system with slight apparent exceptions,[1] in the same direction, for their small deviations from one plane, and for their nearly circular orbits. Professor Newcomb at one time considered the probability of the truth of the hypothesis so strong as to be beyond the reach of further evidence, until the theoretical shrinkage of the sun be actually measured, or the condensation of another nebula actually observed. It may be remembered that the germ of the idea was given in Kant's "Cosmogony," but that Laplace first worked it out in detail.

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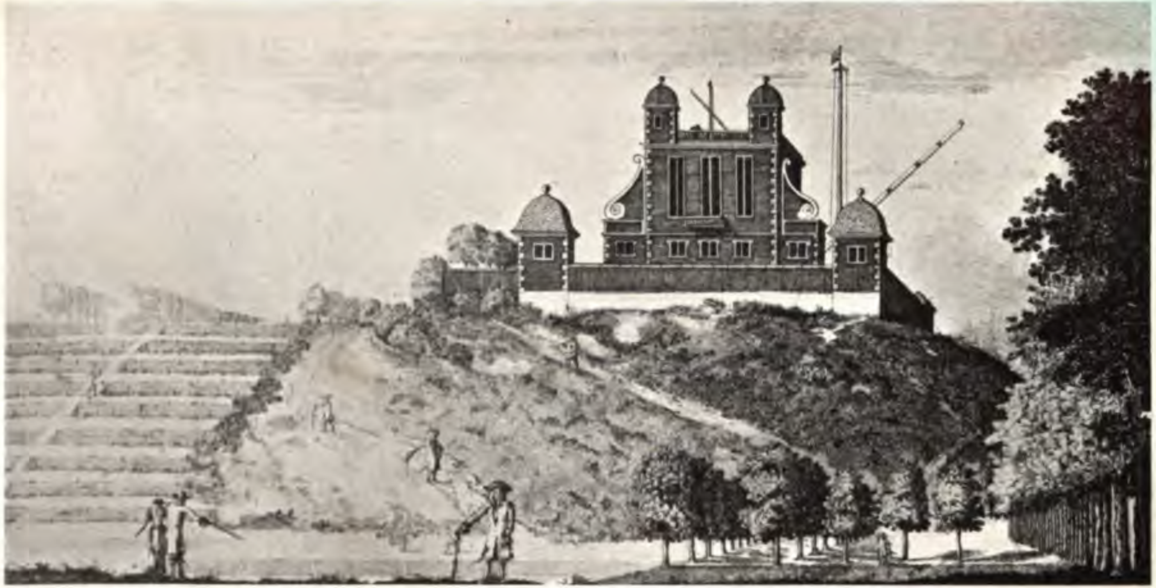
[1] *E.g.* the satellites of Uranus. Comets cannot be considered in the same category, as their original orbits are quite beyond the reach of investigation.



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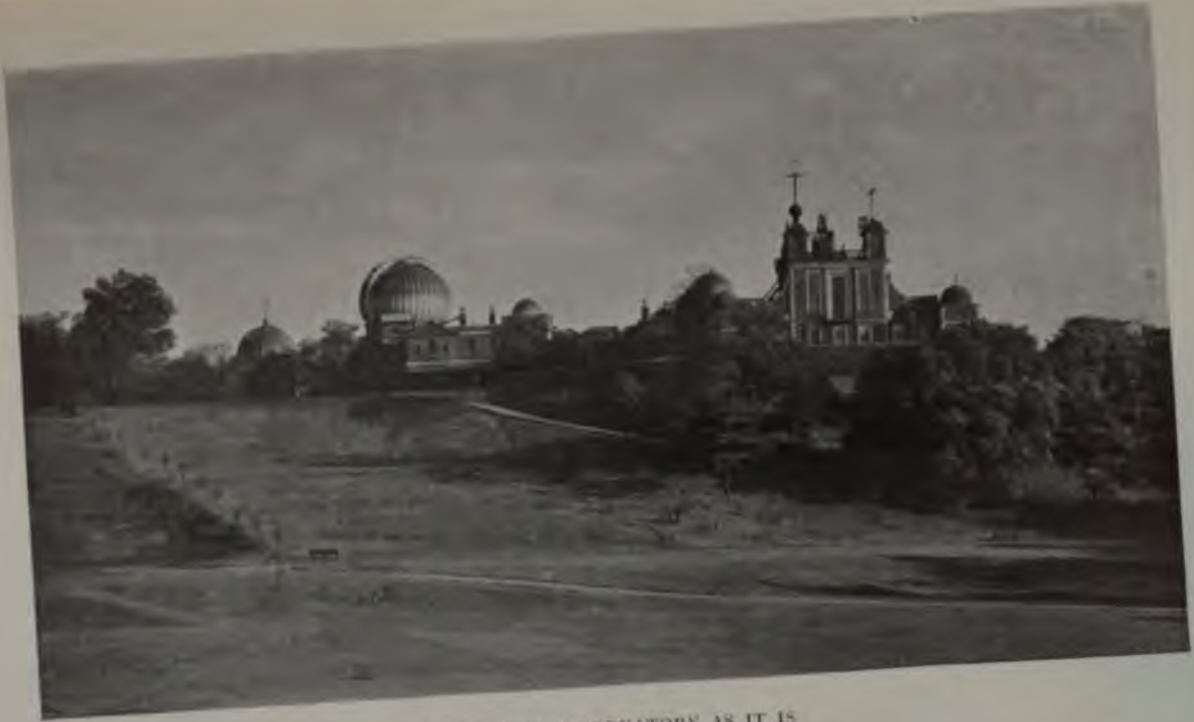

GREENWICH OBSERVATORY AS IT IS

## CHAPTER IX

### FLAMSTEED—HALLEY—BRADLEY—HERSCHEL

WE must now return to other branches of astronomy, which were not neglected while the new physical astronomy was making such progress. Flamsteed has already been mentioned in connection with Newton, and his work must next come under notice. King Charles II., being keenly alive to the importance of the Navy, had concerned himself with various suggestions for determination of longitude, and it was reported that some one had said that lunar tables of sufficient accuracy could not be obtained because the positions of the stars from Tycho's catalogue were not good enough, owing to some erroneous constants employed by Tycho, and to the fact that he had not used a telescope. The King at once ordered arrangements to be made to rectify this by the production of a British catalogue, to be entrusted to the man who pointed out the necessity of it. This man was Flamsteed, and the immediate result of the incident was the foundation of the Royal Observatory at Greenwich, in 1675, and the appointment of Flamsteed to the post soon to be known as that of Astronomer Royal. It is true that the salary allowed him was very meagre, and that most of the expense of his work, including even the provision of instruments and afterwards of skilled assistance, fell mainly upon his own pocket or those of his friends, but students of history will be inclined to lay the blame of this less on the King than on the low standard of public morality in money matters. Flamsteed's ideals were high, and, feeling sure as he did that with adequate instruments he could do work satisfactory to himself, it is not surprising that he chafed at the poverty of his equipment, and was disinclined to rush into print with his earlier observations. There seems also to have been an epidemic of indiscretion among many of the leading mathematicians of the time, which gave rise to much bitterness between Flamsteed and Newton, and between Flamsteed and Halley. Flamsteed was for a long time unable to procure a reliable meridian instrument wherewith to obtain fundamental places of stars by which his sextant observations could be reduced. He knew better than most of his contemporaries the necessity for these fundamental places, not depending on Tycho's catalogue and tables. Newton does not seem quite to have appreciated the importance of this, but for his purpose observations of the moon and planets were more desirable than those of stars, and these to a very fair number Flamsteed provided. Halley seemed to be more concerned with obtaining immediate credit for work done, and was all for quick publication. His own catalogue is almost worthless on that account. There are many modern instances of the same different points of view, one party maintaining the expediency of making sure of anything new before publishing it, risking a possible loss of credit for priority, to avoid loss of credit for reliability; the other maintaining, with great plausibility, that for the general good any delay is inexcusable, as errors are discovered more quickly by many than by one, and that in any case there is something to show for your work. There are many more arguments on each side, but it is sufficient for our purpose to refer to this as the probable cause of much of the friction undoubtedly existing at the time. Flamsteed suffered, moreover, from very bad health, and was also convinced that Halley was a confirmed plagiarist, and that nothing was safe in his hands. It seems to us now that, on the whole, Flamsteed was very badly treated, but there may have been many facts, unknown to us, which would modify that view. Certain it is that he did a great work for English astronomy, and was well worthy of his place at the head of the list of Astronomers Royal. His British catalogue of stars was a very great advance on anything of the kind then extant, and his observations were of untold value for the testing of Newton's theory; though, as he bitterly maintained, Newton made very little acknowledgment of it in his work. It was long before regular systematic observations were made anywhere but at Greenwich, so it will be well to follow for a while the course of events there. It is unnecessary to go deeply into the vexatious delays in the publishing of Flamsteed's "Historia Cœlestis." Queen Anne's husband, George of Denmark, who had undertaken to pay the expense of it, died before any parts were issued; but the publication had been entrusted to referees, of whom Newton was one. When Newton's patron, Lord Halifax, fell from power, to be succeeded by a friend of Flamsteed's, the latter got possession of the remainder of a spurious edition of part of his work, which Halley had brought out without Flamsteed's authority, and in a very imperfect form. Flamsteed destroyed the offending portions of most of the edition, but with failing health and advancing age, the delays had put it out of his power to revise thoroughly, and the work was published after all with many errors and imperfections.

Newton had hinted at Halley being his own successor, but it was to Greenwich that Halley came after the death of Flamsteed in 1720. It is unnecessary to do more than refer to his early labours. His detection of the variation of the compass while still at school, and his improved method of finding the elements of planetary orbits soon afterwards at Oxford, marked him as a brilliant young man. Recognising the need of accurate places of stars in his planetary investigations, and finding that Hevelius and Flamsteed were steadily providing these for the northern sky, he obtained interest with King Charles which enabled him to go to St Helena to observe southern stars. The catalogue he made there suffered from the bad weather, and also from the defect alluded to on p. 64; but on his way he noted the variation of gravity as shown by the pendulum on approaching the equator, and in St Helena he observed a transit of Mercury, which suggested to

HALLEY (1656-1742)

BRADLEY (1692-1762)

him the method of determining the sun's parallax from observations of the transits of Mercury and more especially of Venus. His visit to Hevelius we have already referred to, as also his connection with the pushing forward and publication of Newton's great work. As the result of a two years' voyage, he published the first general chart of the variation of the compass, and was soon after appointed Savilian Professor of Geometry at Oxford. He was sixty-four years of age when he succeeded Flamsteed, but undertook at once a series of lunar observations to extend over a whole revolution of the nodes (rather more than eighteen years), which he brought to a successful conclusion. He discovered the long inequality of Jupiter and Saturn, which, as we have seen, provided such a problem for the continental mathematicians, the acceleration of the moon's mean motion, and the proper motion of the stars; but he is best remembered, in general, by his application of Newton's theory to the comet of 1682, which resulted in his obtaining for it an elliptic orbit and predicting its return about 1759. The fame accruing to his memory on this account goes a long way, it has been well remarked, to compensate the great expenditure of time and money undertaken in consequence of his enthusiastic faith in Newton's great discoveries. He died in 1742 at the advanced age of eighty-five, and made way for a brilliant successor, whose achievements in practical astronomy have placed him in a very high niche of the temple of fame.

James Bradley was born in 1692, and under the care of his uncle, the Reverend James Pound, of Wanstead, one of the best observers of his time, early acquired the practice of careful and accurate observation. Since the discovery by Roemer in 1667 that light did not travel instantaneously, inasmuch as the eclipses of Jupiter's satellites were observed relatively earlier when Jupiter was nearer the earth, it had been constantly suggested that if the earth really traversed such an enormous path, nearly 200,000,000 miles across, in the course of a year, then at any rate some of the stars ought to show a displacement, or parallax, due to the slight change in direction in which they were seen at different times of the year. Flamsteed, among others, had sought to establish the fact by observation, and had found that the pole star did in fact vary its apparent position in the course of a year; but Cassini and others had already pointed out that the motion, whatever its cause, was not due to parallax. Bradley, in order to free his results from uncertainty owing to refraction, selected a star very near the zenith, $\gamma$ Draconis, and had an instrument set up on purpose to observe it. The results at first were not encouraging, for the star certainly had a slight daily motion, in the opposite direction with respect to the pole, to that which could be caused by parallax, and for some time Bradley endeavoured to account for the facts by supposing a motion of the earth's axis. Examining other stars he found that some had an annual variation in latitude only, some in longitude only, and others described an elliptical path, as both latitude and longitude varied. Since all of them, however, seemed to show an annual period, he concluded that it was the earth's motion round the sun that really caused the apparent motion. A lucky chance put him on the right track. Once in the Thames he happened to be on board a vessel which carried a vane at her masthead. He noticed that the vane appeared to show a change of wind with every tack in the vessel's course, and was told that this was not a coincidence but a general rule. He at once saw that the *combined* effect of the velocity of light and that of the earth in its orbit would cause just such an annual effect as he had observed. When put to the test this phenomenon, called aberration, became apparent in the observations; but there was still something left unaccounted for in the observed displacements. Once more Bradley set to work on the same star, and, after allowing for the effect of aberration, found a residual error which increased for about nine years, and then diminished for the same period. This manifestly suggested the primary cause to be the varying position of the moon's orbit, which has a period of rather more than eighteen years, and the immediate effect of which is to cause a slow nutation of the earth's axis in the same period. Bradley determined the dimensions of the apparent ellipse traced out by the earth's pole to be about 18 seconds of arc by about 16 seconds. These two famous discoveries, which assure the place of Bradley among the very first astronomers of all ages, were by no means the whole of his contribution to astronomy. The first was published in 1728, and the second, for which a whole lunar period was necessary, in 1748; but before the latter date, on Halley's death in 1742, he had succeeded him at Greenwich as Astronomer Royal, having been already for some years Savilian Professor of Geometry at Oxford. The monumental work of Bradley at Greenwich, the foundation of accurate stellar astronomy, was long almost ignored, until his numerous observations were collected and reduced by the celebrated Bessel, forming the "Fundamenta Astronomiæ," published many years after Bradley's death, which occurred in 1762.

Meanwhile Lacaille in 1751 had sailed from France to the Cape of Good Hope to determine the parallax of the sun by observations of Mars and Venus, simultaneous with others made in Europe, and to form a catalogue of southern stars not visible in high northern latitudes. He observed more than 10,000 stars in a year, and made many observations of the moon, from which, in combination with those of Lalande, who was at the same time observing at Berlin, the moon's parallax could be directly determined from an arc of 85° on the earth. The principal results of French investigations at this time were obtained in connection with geodesy, arcs being measured in Peru and Lapland to determine the difference of curvature, and many pendulum observations made to determine the variation of gravity, and the effect of mountains on the vertical studied by means of the plumb-line.

Maskelyne, who became Astronomer Royal some u o

LAPLACE (1749-1827)

W. HERSCHEL (1738-1822)

two years after Bradley's death, is celebrated for the voyage to St Helena which he made to observe the transit of Venus in 1761. The voyage was in many ways a failure owing to bad weather and instrumental defects, but it gave Maskelyne an opportunity of practising the method of finding longitude at sea by lunar distances, which he subsequently recommended to the Admiralty. Tables for this method were subsequently issued in the "Nautical Almanac," which for forty-eight years was published under his direction.[1]

One great name remains before the close of the eighteenth century, Sir William Herschel, the pioneer of modern descriptive astronomy. Born in Hanover in 1738, and earning a modest living as a musician at Bath, he devoted his leisure to making larger and larger reflecting telescopes and exploring the sky with them. In 1781, while thus employed, he observed what appeared to be a star with a sensible disc, which was enlarged by the use of a high-power eye-piece. At once concluding that it could not be a star, he continued his observations and found a slow motion, from which he assumed it to be a comet, and sent word to Maskelyne. It was soon found, however, that the object did not vary its distance as a comet would, and that it must be a planet. Herschel, in compliment to his patron George III., called it Georgium Sidus, while continental astronomers called it Herschel in honour of the discoverer. It has now, however, for a long time been known only as Uranus, though the conventional symbol for it, ♅, still recalls its discoverer. Flamsteed and others were soon found to have already observed it as a star, and from their observations Delambre constructed an orbit for the new planet. Herschel subsequently discovered two satellites belonging to Uranus, and suspected four more, and remarked many new features of Saturn, including two satellites which had escaped the scrutiny of Cassini and Huyghens. Moreover, by the aid of his large telescopes he opened up new fields for observation, describing a large number of nebulæ, and classifying many double stars. He also from the observed proper motions of certain stars came to the conclusion that the solar system was travelling through space in the direction of a point in the constellation of Hercules; or rather, that it was travelling in a mighty orbit, and that its motion was for the time being in that apparent direction. His only assistant was his devoted sister Caroline, who, besides being at his beck and call while at work, and also acting as his amanuensis and preparing charts for him, found time to discover eight comets; and, after her brother's death in 1821, laboured to prepare his observations of nebulæ and clusters for publication.

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[1] This method has, since the great improvements in chronometers, become of almost entirely academic interest, providing examination questions for naval and mercantile officers, and the "Nautical Almanac" is ceasing to print the tables.

# CHAPTER X

## THE EARLY 19TH CENTURY—NEPTUNE

AT the end of the 18th century, regular systematic observations were taken and published in their original form only at Greenwich. Lalande's "Histoire Céleste," contained thousands of observations of small stars mostly made at the Observatory of the Ecole Militaire, but most continental observations appeared only in the various national ephemerides, and occasionally in the transactions of learned societies. But in the year 1800 a new epoch began with the commencement of Zach's *Monatliche Correspondenz*, destined to make known regularly all that was being done in the astronomical world. Its publication, like most continental scientific work, was interrupted in 1813, but it had successors, one of which, the "Astronomische Nachrichten," founded by Schumacher in 1821, still holds the leading place as an international channel for the communication of discoveries and observations. This new development came none too soon, for on the first day of the 19th century, Piazzi, at Palermo, a careful astronomer who took the invaluable precaution of repeating all his observations after a comparatively short interval, discovered a star-like object in apparent motion, which proved to be a small planet with an orbit between those of Mars and Jupiter. This he named Ceres, and Gauss' new "Theoria Motus" gave its orbit with such accuracy that it was easily found again, after having passed through a large part of its orbit in which it was invisible owing to the proximity of the sun. The Germans had long professed to expect a planet at a distance from the sun intermediate between those of Mars and Jupiter, to fill a gap in the empirical law of Bode or Titius, connecting the distances of successive planets from the sun, a law obeyed approximately by all the then known planets, including Uranus. The minor planets or asteroids, of which this was the first, now number some six hundred, but the rate of discovery was at first slow.

The second, Pallas, was discovered by Olbers in 1802, and from the circumstances that the major axis of its orbit was nearly equal to that of Ceres, and that their orbits were near each other at the intersection of the orbital plane, sprung the hypothesis that they were fragments of a large planet and that other portions might be found with orbits passing near the same points of intersection. Every month he examined a certain portion of the heavens round one of these points, and was at last rewarded in 1807 by the discovery of Vesta; but meanwhile in 1804 Harding had discovered Juno near the other point of intersection. More than thirty-eight years elapsed after the discovery of Vesta, before Hencke added a fifth, Astræa, and by that time Piazzi, Harding and Olbers were dead. In 1847 three more were discovered, Hebe by Hencke, and Iris and Flora by Hind, afterwards superintendent of the "Nautical Almanac," and since then the number has gone on increasing, at first steadily, but since the application of photography to the search, by leaps and bounds. The problem of finding suitable names has become one of some difficulty, in fact one is inclined to think, looking at some of the names that have been assigned, that the task has proved impossible. But we must return to this development later, merely noting that in the middle of the 19th century the number of asteroids known was thirteen. The crowning glory in planetary discovery, however, was the prediction and finding of Neptune. John Couch Adams in 1841, while working for his Tripos at Cambridge, came across some unexplained anomalies in the motion of Uranus. Others had considered the problem thus presented, among them Bessel, whose plans for investigation were cut short by death; Adams made a note of the problem, to be tackled after his Tripos, and as soon as he had gained the distinction of Senior Wrangler in 1843, he returned to the consideration of the anomalous inequalities of Uranus, with the avowed intention of testing the possibility of their being caused by a planet still more remote. All the known observations of Uranus, dating back to the time of Flamsteed, had already been compared by Bouvard, who could not find elements that would satisfy all the observations, and when he obtained approximate success with the more recent observations by rejecting the old ones, it was found in the course of a few years that the predicted places were gradually receding from the truth again. While Adams was applying to Airy, the Astronomer Royal, for Greenwich observations of Uranus, Arago, the director of the Paris Observatory, was urging Le Verrier to undertake the same problem. In the autumn of 1845 Adams arrived at an approximate solution of the inverse problem in perturbations to which he had devoted his attention, and finding it represented the anomalies in longitude fairly well, sent it to Airy, who, before paying much attention to it (as was only natural from the diffident way in which Adams presented his result), desired to know whether the errors of the radius-vector would be equally represented by the suggested solution. Adams, by failing to reply for some months, threw away the advantage of having reached his result so early, for in the meantime Le Verrier had also arrived at a hypothetical orbit for the disturbing planet. It thus happened that Airy had both sets of elements before him and asked Le Verrier the same question regarding the radius-vector. Le Verrier at once assured him that his elements were bound to satisfy all the discordances, and was so confident of his success that he asked Dr. Galle of Berlin to look for the planet in a definite assigned place, Galle being provided with a new set of star maps constructed to facilitate the recognition of minor planets; and on the first evening of the search the planet was found very near Le Verrier's predicted place. Professor Challis at Cambridge meanwhile had been trying, without much hope of success, to locate the planet from Adams' predicted place, and he had actually succeeded in observing it more than once, but being unprovided with the star maps, continued his observations over a larger tract of sky before reducing the earlier ones for comparison. Thus it was that the applause of the world was first showered on Le Verrier alone, and Adams' equal claims only obtained later recognition. Too much has been written on the vexed question of the responsibility for the delay in working from Adams' results, but it is generally conceded that he himself was at least as much to blame as anybody. It was soon found on constructing an Ephemeris that, except Lalande, no one was likely to have observed the planet before, and a diligent search resulted in the discovery of two observations in 1795, which were of great value in computing the orbit of the new planet, which, although at first called Le Verrier, soon became generally known as Neptune. Into the further discussion as to whether the planet discovered was really the planet indicated by the theory of Le Verrier, inasmuch as its elements differed considerably from the predicted ones, we need not enter. It is sufficient that the discovery was predicted and the inverse problem in perturbations proved approximately soluble. All honour therefore to the genius of the two men who attacked and solved it. Neptune's mean distance was a severe blow to the empirical law of Bode above referred to, being far too small to satisfy it. Uranus had fitted very badly, Neptune refused to fit at all.

Meanwhile an Englishman, William Lassell, had made for himself a reflecting telescope, ingeniously arranged with an equatorial mounting and of such good definition that he discovered that Neptune had a satellite, observations of which enabled the planet's mass to be calculated. He turned his attention to Saturn, whose known satellites already numbered seven, one discovered by Huyghens, four by Cassini, and two by William Herschel, and in 1848 found an eighth satellite, also discovered at the same time in America by W. C. Bond of Harvard, who two years later discovered what is known as the "Crape" ring, a dusky ring within the inner portion of Saturn's bright ring.

It was not until 1851 that Lassell certainly detected the two inner moons of Uranus, and it is probable that these were not seen by Herschel and that his four doubtful ones have no real existence. The mention of Lassell's equatorials with which he did such good work, both near Liverpool and in Malta, brings us to other instrumental improvements of the period.

England had long held a high place for accurate instruments. Abraham Sharp, who divided Flamsteed's great quadrant, Graham, with whose instruments Bradley made most of his observations, and Bird had brought the art of graduating quadrants to a high degree of accuracy. In 1809 Edward, one of the brothers Troughton, devised a plan for graduating complete circles, and three years later the first mural circle was set up at Greenwich. A reversible circle was already known, for Piazzi had one at Palermo in the form of an altazimuth, and there was a small one at Greenwich. With these the zenith distance of a star on the meridian was found by quickly reversing the instrument about the vertical axis and taking two observations as near the meridian as possible, determining the error of the vertical axis by a plumb-line. The new mural circle, not being reversible, could not be used in the same way, but with it polar distances could be measured, the polar setting being found from observations of stars above and below pole. Pond, the Astronomer Royal at the time in succession to Maskelyne, introduced about 1821 the method of taking observations of the same star at the same meridian passage in two parts, one direct and the other by reflection at a trough of mercury, which enabled the zenith distance to be determined. The next step was to combine the two meridian instruments, the transit and the mural circle, so as to enable the same object to be observed in both right ascension and polar distance or zenith distance by the same observer at the same time. Smaller instruments of this kind were gradually being introduced, and in 1850 the Greenwich Transit Circle was set up, which is still in constant use. Meanwhile, clock-work motion for equatorials was also introduced, which enabled a celestial object to be followed easily, the motion of the clock counteracting the effect of the earth's rotation and keeping the telescope pointed in the same direction in space. Improvements in the optical portion of telescopes were also keeping pace, the apertures increasing to nine, then eleven and twelve inches, this advance having been rendered possible by the invention by Dollond in 1758 of the achromatic lens, the principle of which was by the combination of two lenses of different kinds of glass and of different dispersive power, to counteract approximately the effect of the spreading of colour fringes, due to the fact that the focus of a single lens is not the same for all colours, the dispersion of the convex lens being nearly neutralised by the opposite effect produced by a concave lens, of such different glass, however, that the combined effect was still that of a convex lens, in order not to lose the magnifying power. The great advance thus rendered possible was very largely made by Fraunhofer of Munich, whose first great success, a fine objective of 9½ inches in diameter and 14 feet focal length was long known as the great Dorpat refractor. He also constructed for Königsberg Observatory the first effective heliometer. The principal feature of this celebrated instrument is a divided object glass, the two halves of which can give separate images, the amount of the motion given to the moving portion being strictly measurable by the screw which moves it; so that the diameter of a celestial disc or the angular distance between two celestial objects can be found by separating the images until one pair of opposite parts coincides. One of the greatest advances in exact astronomy, however, was the gradual adoption of the principle of determining and correcting residual instrumental errors, instead of trying to reduce them to zero by continual adjustment of the instrument.

But while the new astronomical periodicals above referred to were doing yeoman service, especially in the matter of the speedy dissemination of news of discoveries, they were by no means the only signs of growing activity. In 1814 Bessel commenced the regular publication of the Königsberg observations, and Struve those of Dorpat, followed in 1820 by the first volume of Vienna observations. The same period saw Piazzi's great catalogue of stars, and the beginning of Argelander's work at Åbo. The United Kingdom also was not content with steadily increasing the staff and efficiency of Greenwich Observatory. The Dublin Observatory under Brinkley became active, though not attempting systematic publication. In 1823-24 the Cambridge Observatory was erected, and regular publication commenced with the advent of Airy in 1828, though he is far better known by his long tenure of the post of Astronomer Royal, in which he succeeded Pond in 1835. Soon afterwards regular publication was commenced under Dr Robinson at Armagh. In order to supplement the fundamental work of Greenwich by a southern observatory, the British Government founded one at the Cape of Good Hope, which was completed and equipped in 1829. Paramatta Observatory in New South Wales was founded and equipped by the Governor, Sir Thomas Brisbane, in 1822. On the continent of Europe also observations were already growing numerous; and soon afterwards the impetus given by some early successes at Cincinnati, Harvard College, and elsewhere in the United States, inaugurated a period of activity there which has been ever since increasingly maintained, except during the period of the Civil War.

In 1820, moreover, was founded the Royal Astronomical Society (first known as the Astronomical Society of London), whose Monthly Notices and Memoirs are still a leading feature in astronomical publications.

# CHAPTER XI

## HERSCHEL—BESSEL—STRUVE

IT was in 1818 that Bessel's great work already referred to appeared, containing Bradley's stellar observations uniformly reduced to the epoch 1755. Five years previously he had published the table of refractions from Bradley's observations, which he now improved with a revised theory; and in 1821 and 1822 published, in the Königsberg Observations, a number of his own observations of stars near the horizon to correct the tables still further. These latest results succeeded in getting rid of a curious anomaly, by which nearly every astronomer who determined the obliquity of the ecliptic from the summer solstice obtained a greater value than from the winter solstice. Though various suggestions had been made to account for this, it was generally felt to be due in some way to refraction, and Bessel's new tables seemed to be conclusive on the point inasmuch as with them the anomaly disappeared.[1]

Other important catalogues remaining to be noted in addition to that of Piazzi in the early part of the century, are one of circumpolar stars, epoch

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[1] There is still a little uncertainty attaching to determinations of constants depending on solar observations, a very recent suggestion being that the use of coloured shades in observing the sun causes a small systematic difference in the result.

1810, by Stephen Groombridge, who observed at Blackheath, Bessel's observations, divided into two zones, of declination −15° to +15°, and +15° to +45°, known as Bessel's First and Second Catalogues, epoch 1825, and Argelander's northern zones, declination +45° to +80°, epoch 1842.

Stellar parallax was still being diligently sought, Piazzi, Brinkley, and others claiming success which Bessel at Königsberg and Pond from the Greenwich observations were unable to confirm. We have seen how the search for this evidence of the truth of the Copernican theory had resulted in the great discoveries of Bradley; besides having probably contributed to the first determination of stellar proper motion by Halley. It must also be noted that Herschel by investigations in a different direction with the same object in view, was led to the opening of a new branch of astronomical research. It was not at that time considered possible that stars should have any physical connection. The apparent closeness of the components of such a star as Castor was held to be accidental, and it was assumed as almost an axiom that the fainter of two stars was necessarily the more remote, the underlying assumption that all stars are really equal, in itself inherently improbable, not seeming to occur to any one. Instead of multiplying fundamental observations of a few stars right through the year, in order to detect any variation in their relative positions which might be ascribed to parallax, Herschel chose the much simpler plan of comparing the relative positions of pairs of stars, arguing that the brighter star, being supposed nearer, would show a parallactic displacement relative to the fainter, and that by using stars so apparently near together as to be visible in the same telescopic field, micrometric measures of the angle and distance would detect such displacement without the labour of determining the fundamental places. Some of the pairs examined by him did indeed show a relative displacement, but so far from this being an annual effect due to parallax, it became before long increasingly evident that it was an actual relative motion of the stars themselves, that by continued observation and measurement the period could be determined, and that the motion was such that the scope of Newton's laws could be extended to them, far beyond the limits of the solar system, inasmuch as in every one of these distant systems equal areas were described in equal times. Such was the origin of double star astronomy, a field entered with avidity by the celebrated Struve as soon as he was equipped with the great Dorpat refractor already mentioned. More than three thousand pairs of stars were catalogued by him, and a large number added by his son Otto Struve, so that the new branch established by Herschel was in no danger of neglect. Herschel himself was not content with this development; he thirsted to discover the origin and development of the systems seen in his telescopes, and following, though independently, the notion already suggested by Kant and adopted by Laplace and others, turned his attention to nebulæ and clusters to search for evidence of progressive change. In the object sought he was not more successful than before, but the result of his labours in this field is of immense value, the catalogues laboriously prepared and published being of fundamental importance to his followers in this branch. His first assumption, that nebulæ were miniature "milky ways" and their appearance caused by close grouping of stars, was gradually abandoned when he found that though some nebulæ were resolvable, others were not, and that the light was of a different character from starlight, a result afterwards confirmed by the spectroscope. His division of the nebulæ into classes was part of his scheme of evolution, from diffused nebulosity, through greater and greater apparent condensation down to planetary nebulæ assumed to be already solidifying in the centre; and this theory has not yet been superseded, though the successive steps are too slow apparently for us to expect direct evidence of its truth.

One other branch of sidereal astronomy, that of variable stars, was still in its infancy. With the exception of the celebrated "new stars" to which in all probability we owe the catalogues of Hipparchus and Tycho Brahe, no variations in the heavens had been recorded other than those of the members of the solar system, with which we may now rank comets, since 1759 saw the verification of Halley's prediction of the return of the comet of 1682. Fabricius in 1596 noticed a star which three months later was invisible. It was however catalogued in 1603 by Bayer as o Ceti, and soon discovered to be periodically fluctuating. Hevelius called it "Mira," the wonderful, and in 1667 Bouillaud determined its period of variability at 334 days. Before this another variable had been discovered in Cygnus, and soon afterwards, in 1669, the celebrated Algol joined the group, destined, however, to give the name to a new class of variables, inasmuch as the loss of light is held to be due to the interposition of a dark body revolving about the bright star. By the end of the 18th century a few more were known, but it needed the impetus given by Herschel's wonderful success in pointing the way to fresh fields open to amateur observers to establish this branch also.

It was well-nigh inevitable that Herschel's systematic survey of the heavens should put him on the track of variable stars, in addition to the other directions in which his genius and application bore fruit. One star noted by him, 55 Herculis, has apparently disappeared completely, but one important variable, α Herculis, is a discovery of Herschel's and is also of great interest in itself, inasmuch as it held an intermediate place between two different classes of variables into which the few then known were obviously divided, short-period variables which went through their cycle of variation in a week or less, and long period variables whose cycles ranged between 300 and 500 days approximately. On Herschel's conjecture that variability might be caused by rotation, one part of the surface of a star being of different brightness to another, it seemed inexplicable that the periods should be so very unequally distributed; hence the discovery of α Herculis with an apparent period of about sixty days was exceedingly welcome, as tending to bridge the gap. It appears now, however, that this period is illusory and that the variation is irregular. In addition to the survey of the stars, Herschel also scrutinised the surfaces of the moon and planets, noting some peculiarities in the former, which he described as lunar volcanoes, and taking special care with respect to Saturn among the latter; it is to him we owe the first announcement that Saturn is not spherical, and that its outer satellite, Iapetus, like our own moon, turns always the same face towards its primary. He was probably also the first astronomer who considered his eye as part of the observing instrument, and was careful to adjust the position of his head in order to view such an object as a band on a planet in the same direction relative to the position of the retina; and there is ample evidence that these precautions are of great importance, personal equation in various forms being one of the most elusive and widespread varieties of systematic error.

Herschel, however, was not the only genius at work in the early days of the century. We have alluded more than once to Bessel in connection with the reduction of Bradley's observations undertaken at Königsberg. His training was, as seems to be almost the rule rather than the exception, strictly unscientific; he was, in fact, employed in a mercantile house, but wishing to qualify for the post of supercargo on one of the trading expeditions to the East, he was led from foreign languages to

BESSEL (1784-1846)

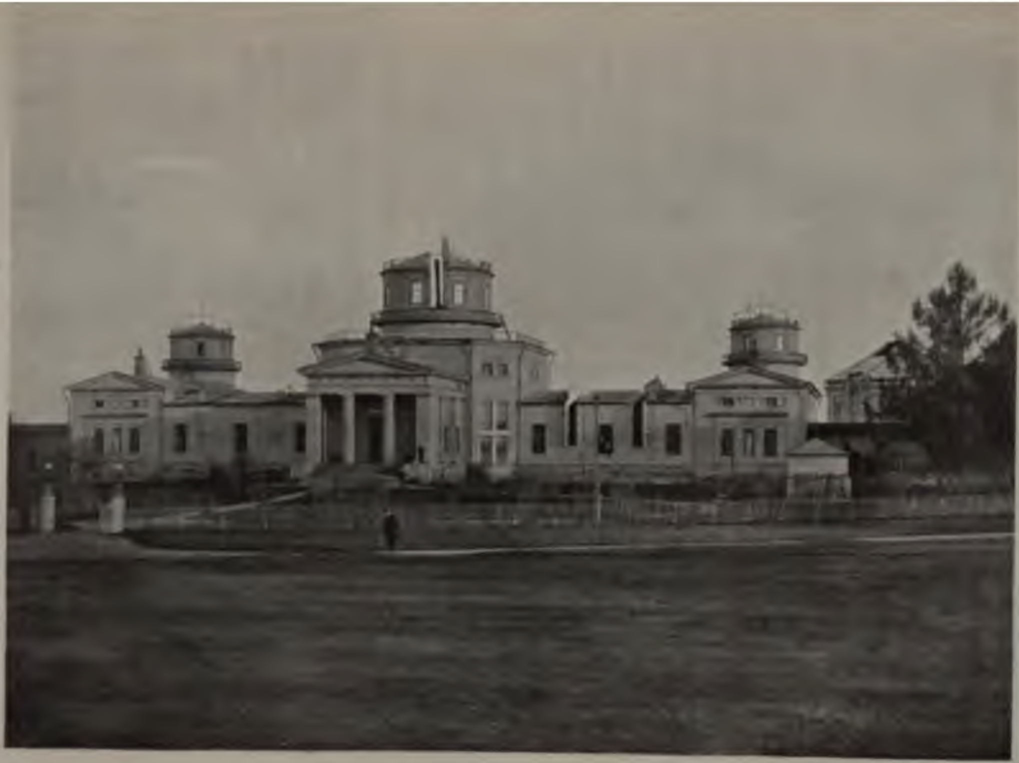
W. STRUVE (1793-1864)

geography and thence to navigation and nautical astronomy, and finding delight in the new pursuit, he set to work eagerly to study mathematical astronomy, and at the age of twenty deduced from the observations of Harriot of the apparition of Halley's comet in 1607 an orbit of that body which he submitted to Olbers, the discoverer of Pallas and Vesta, who was also noted for cometary research. Olbers at once sent the paper for publication in Zach's *Correspondenz*, and made known the coming astronomer whom two years later he persuaded to give up his business career in order to succeed Harding, the discoverer of Juno, who had just been promoted to Göttingen from his post of chief assistant to Schröter at Lilienthal. Four years later Bessel was chosen to superintend the new observatory at Königsberg, then only being erected by the Prussian government, where he worked from 1813 to 1845. By his reduction of Bradley's observations he practically put back the date of his improvements to 1755, and enabled the proper motions of many of the stars to be determined. His improvements, consisting of the accurate determination of the corrections for refraction, aberration, precession, and nutation, were embodied in his "Tabulæ Regiomontanæ" (Königsberg Tables). His catalogues already referred to were intended to supply a third fiducial epoch for comparison with those of Bradley (1755) and Piazzi (1800), and that of Argelander was also indirectly due to him, for it was Bessel who trained and made an astronomer of Argelander, whose greatest contribution perhaps was the celebrated Bonn Durchmusterung, or survey of the whole of the northern heavens in zones, made with a small instrument in order to have a large field of view available and get over the "ground" quickly. This work contains approximate places and magnitudes of more than 300,000 stars, and is accompanied by a set of maps containing the positions of all of these stars, both for the epoch 1855. Another notable achievement of Bessel's was the first real determination of stellar parallax. In his work already referred to he had deduced proper motions for several stars common to Bradley's and Piazzi's catalogues, and having come to the very reasonable conclusion that the stars which showed most proper motion, whether due to their own motion or to that of the solar system, were in all probability the nearest of the stars, he determined to search for parallax in the star that had the greatest known proper motion. This was a double star, 61 Cygni, noted by Piazzi as having a proper motion of more than 5 seconds of arc, which Bessel himself confirmed twenty years later. In 1837 he found time to devote the Königsberg heliometer (already mentioned) to the problem, and soon deduced a parallax of one-third of a second of arc. The value found was too small, but it was the first direct success in the field so long ploughed, and so fruitful in indirect and unexpected results. Struve at the same time was working at the same problem in connection with Vega (α Lyræ), with the Dorpat telescope, but his result, which was published after Bessel's, was three times too great. Before even

Bessel's result was published another star, beyond his reach, α Centauri in the southern sky, had been proved to possess a still larger parallax. Henderson, for a short time director of the Cape Observatory before being appointed Astronomer Royal for Scotland, brought back with him a series of observations of this star; and learning that it had a large proper motion, examined the observations for a possible parallax, deducing one of a second of arc. He waited so long for confirmation that Bessel's result was published two months before his communication to the Royal Astronomical Society—in January 1839. The distances of the stars thus determined are so immense that for convenience they are not measured in miles or even in millions of miles, but in "light-years," a light-year being the distance travelled *in a year* by light, which flashes at a speed of ~~more than~~ 186,000 miles *in a second*. Measured by this immense unit the distance of α Centauri is more than 4, that is, if its light were suddenly extinguished we should not know it for more than four years. Yet many of the brightest stars have as yet shown no parallax, and are probably at least a hundred times as far away as α Centauri, while some groups have been estimated to lie at a distance of some 3000 light-years. Another great achievement of Bessel's was the discovery of the orbital motion of the two Dog-stars, Sirius and Procyon, which, in 1844, after a very refined series of measures, he pronounced to be revolving about dark or relatively dark companions. His death in 1846 forestalled the confirmation of his announcements, to which we shall refer later.

His great contemporary, F. G. W. Struve, already mentioned as an ardent double-star observer, worked first at Dorpat from 1813 to 1837, and afterwards at the new observatory at Pulkowa, established on a lavish scale by the Czar Nicholas, who desired to eclipse every similar institution then existing, and procured what was long regarded as the finest instrument in the world, the great 15-inch refractor from the workshops of Fraunhofer's successors (Merz and Mahler). We cannot too highly value his labours in the then novel field of double-star observation. Unlike Herschel, who, as we have seen, was hunting for parallax and observed "wide" pairs, Struve confined his attention to stars separated by not more than 32 seconds of arc. He concluded that an appreciable percentage of stars, more especially brighter ones, are provided with companions, and that physically connected groups of three, four, or more stars undoubtedly exist. After nineteen years of activity at Pulkowa, from the completion of the observatory in 1839, his health gave way entirely, and though he lived until 1864, his son Otto was practically director from 1858.

Herschel's work required completion for the southern sky, and even in the northern part the extensive field in which he laboured did not seem to attract a successor among the leading astronomers of the time. This honour was reserved for his son John, who quitted legal studies to take up



PULKOWA OBSERVATORY

double-star astronomy in conjunction with South. About the time of his father's death the two were engaged on a series of measures of some hundreds of binaries, which in many cases afforded striking proof of the correctness of the inference of orbital motion. In 1827 Savary, of Paris, demonstrated the applicability of Newton's law to ξ Ursæ Majoris, and since then great improvements have been made in the method of investigation of double-star orbits with marked success. Between 1825 and 1833 John Herschel systematically surveyed the northern heavens, verifying his father's discoveries of nebulæ and clusters, and adding hundreds more, besides some 3000 double stars. Then, fired with ambition to complete the survey, he transported his family and his reflector to the Cape, where, at Feldhausen, near Table Mountain, he completed his task in four years, the full results being published in 1847. More than 2000 double stars, and nearly as many nebulæ were among the fruits of this undertaking, and much time was also spent in observations of the relative distribution of stars, in continuation of similar work of his father's, to which we shall recur later. He was able also to observe an outburst of the wonderful variable star η Argûs, involved in the Argo nebula. Halley had seen it of fourth magnitude in 1677; Lacaille and others, nearly a century later, of second magnitude. In December 1837, however, John Herschel saw it suddenly three times as bright as before, and a fortnight later it was nearly the third brightest in the sky. It then faded, but was brighter than ever, and second only to Sirius in 1843, its changes and fluctuations being very unsteady and unlike those of ordinary variable stars. It has long been invisible to the naked eye, having faded more or less regularly until 1887, since when a partial recovery has left it a dull red seventh magnitude star. Its period, if indeed it has one, is quite unknown, and its behaviour is still a most perplexing problem, differing on the one hand from "new stars," which suddenly flash out and slowly fade, and on the other from the regularly fluctuating variables. Another branch of work pursued by him was Stellar Photometry. It had before been usual to make all observations of the brightness of stars differential, *i.e.* a star B was said to be fainter than A and brighter than C, and many sets of groups had to be compared in order to arrive at definite values. Herschel, however, compared each star separately with a standard, which was in his case an artificial star formed by moonlight totally reflected from the base of a prism. The distance at which the artificial star appeared equal to any natural star gave a measure of the brightness of the natural star. Nearly 200 stars in the northern and southern sky were thus gauged, and their "magnitudes" set down in terms of that of α Centauri. He also determined the light of the moon in terms of his standard star, and as measures of the comparative brightness of the sun and moon had already been made, it became possible to compute the real brightness of some of the stars.

John Herschel was knighted before his voyage, and made a baronet on his return, but the rest of his life was mostly devoted to the work of cataloguing the vast mass of observations already obtained, and even this as regards the double stars he did not live to finish, though he reached his eightieth year, dying in 1871.

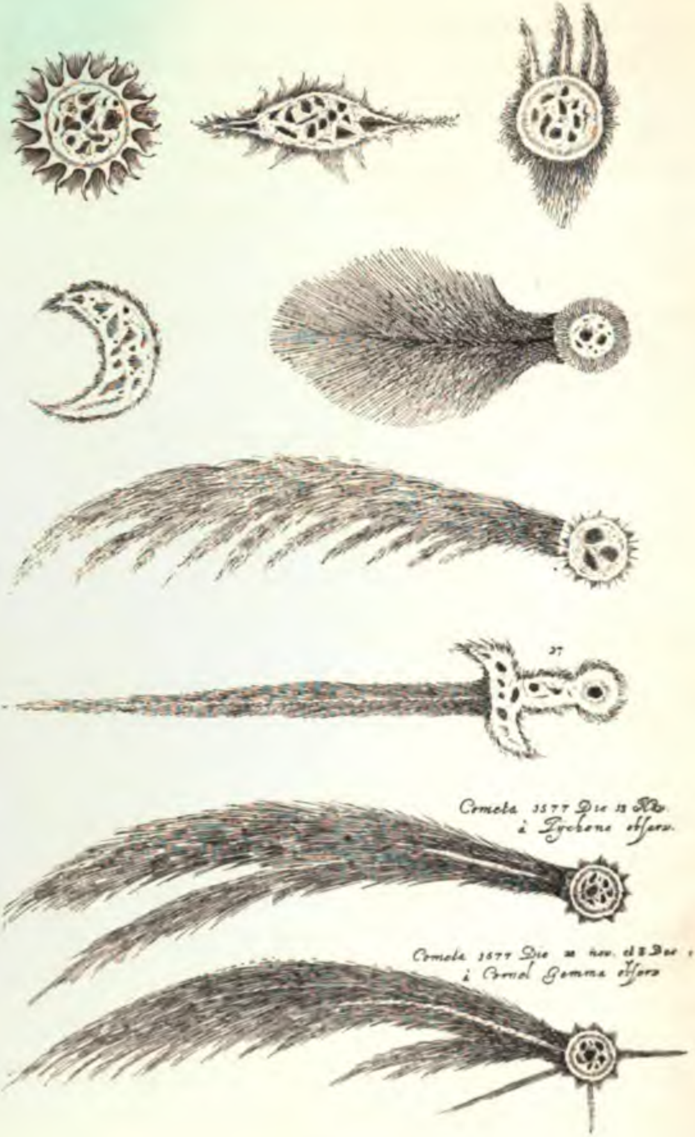
# CHAPTER XII

## COMETS

AND now a digression is once more necessary, for some other branches of astronomy remain to be reviewed before we pass on to more modern times than the first half of the nineteenth century. We have seen how Halley was led to presume the identity of the comet of 1682 with that observed by Kepler in 1607 and Apian in 1531, and to predict its return in 1758-9, thereby bringing comets into their fit place as eccentric members of the solar system, stripping them of the long-sustained character of portents, and affording new evidence of the triumph of law. After this success, the possibility of an elliptic orbit in which a comet might return was kept in view, and any failure of parabolic elements to fit observations was regarded hopefully as a sign of periodicity. The first comet of really short period, however, was not detected until long afterwards. Olbers, whose name has already occurred as scientific sponsor to Bessel, was a physician at Bremen, and devoted most of his scanty leisure to cometary astronomy. An important simplification, known as Olbers' method, by which an approximate orbit could be obtained, was a happy thought that occurred to him during his student days, and not only became, on publication, the usual method employed for such approximations, but also had the effect of directing his energies in the particular direction indicated. For many years he was the sympathetic adviser of contemporary workers, and his chief contributions to the list of discoveries include a periodic comet discovered in 1815, which returned in 1887; two minor planets, Pallas and Vesta; and, greatest of all, Bessel, whom Olbers himself considered as his crowning glory. Other workers in the field were many. We have seen how Caroline Herschel discovered no fewer than eight comets, being set to sweep the sky in horizontal bands for this purpose, in the intervals of her arduous labours as assistant to her brother. Far more prolific was Pons of Marseilles, whose discoveries, some thirty odd in number, include an inconspicuous object seen in 1818, the observations of which were taken in hand by Encke, with the unexpected but gratifying result that its period came out at 3⅓ years, which is still the shortest period known for a comet, and its identity was established with comets observed in 1786, in 1795 (by Caroline Herschel), and in 1805 (by Pons himself). Encke predicted its next perihelion return for 1822 May 24, and it was discovered, close to the predicted place, at Paramatta, being invisible in the northern hemisphere. The value of such a comet to astronomy is great, for it enables further light to be shed on the other members of the system. Encke's comet, as it was called, at perihelion is within the orbit of Mercury, and the perturbation due to that planet enabled its mass to be for the first time computed, the absence of a satellite having previously discouraged any such attempt. The other end of its orbital oval is near that of Jupiter, so that it belongs to what is now known as the Jupiter family of "planetary" comets. Others have been found belonging to Saturn, and the masses of these great planets, especially Jupiter, are corrected by observations similar to that referred to in the case of Mercury. This comet also confirmed a fact, stated by Hevelius, that comets contract on approaching the sun, the contraction at the 1838 return being in the ratio of 800,000 to 1, rendering any doubt as to the actuality of the phenomenon impossible. The physical cause is, however, even now an unsolved problem. Yet another problem was raised by the behaviour of this celebrated comet, for after making due allowance for all planetary perturbations the fact remained that its returns took place too soon. Olbers and Encke assumed the cause to be a resisting medium of extreme rarity, whose effect would be slightly to diminish the centrifugal force, and so to lessen the distance from the sun, thus of course increasing the velocity, just as shortening the pendulum accelerates the rate of a clock. The existence of such a medium would involve in course of time the successive destruction of the planets and comets by gradually drawing them in to the sun, but no other member of the system has been proved to show a similar effect. Encke indeed only assumed the medium to exist close to the sun, or rather to increase rapidly in density towards the sun, so as to have no effect so far away as Mercury. Encke's comet, though fluctuating in brightness, does not yet show any great change, but its acceleration is no longer so great. The next periodic comet to be identified was discovered in 1826 by Biela and identified with comets seen in 1772 and 1805, with a period of between six and seven years. It was remarkable in other ways, first for the scare caused by Olbers' announcement that it would pass through the earth's orbit on 1832 October 29. It only reached that point, however, a month after the earth had passed it, so that the danger was not only very slight (as evidenced by the observation of Sir John Herschel of very faint stars through the substance of the comet without loss of light), but also remote, as the nearest approach to the earth was more than 50,000,000 miles away. Its next remarkable feature was noted at the return of 1845-6, when it divided into two portions which pursued their way independently. It was just seen, the two components much further apart, at the next return in 1852, but never afterwards, at least not in the form of a comet.

There seems no doubt that all periodic comets have become so in consequence of their "capture" by planets, whose influence would tend to draw into the system a comet coming within reach and travelling in the same direction round the sun as the planets, and expelling those whose motion was in the opposite direction. All known periodic comets do, in fact, follow the direction of the planets, that of Halley being associated with Neptune, and others with Uranus, Saturn, or, most of all, with Jupiter.

Halley's comet appeared again in 1835, and having been predicted was discovered early and followed for more than nine months. Of all the "planetary" comets this is the only one that comes near being called a "great" comet; in fact it may fairly be reckoned as a sort of connecting link between the probably dying comets of short period and the great comets met with at long intervals in history, whose orbits are of vast extent and their substance brighter or of higher reflecting power, so that they are easily visible without a telescope and develop enormous tails. Halley's comet, as we have seen, was observed generally in 1835, 1758 and 1682, by Kepler and Harriot in 1607, and by Peter Apian, a diligent cometary observer, in 1531. There is no doubt that it was observed in 1456, in 1378, probably in 1301, and 1223, and almost certainly in 1145. Bright comets, strongly presumed to have been different returns of the same object, have been traced, principally from Chinese annals, as far back as B.C. 11. A little latitude in the interpretation of the Chinese observations, and a slight change in the elements from time to time, are all that is requisite to provide a complete set of twenty-five appearances of this comet, most of which were observed before the invention of the telescope. The next return is expected in 1910. If, as some allege, the comets of 1264 and 1556, and possibly also of 975, were identical, another return should have taken place within a few years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Cometa 1577 Die 13 Nov. à Tychone observ.
Cometa 1577 Die 20 Nov. et 8 Dec. à Corneli Gemma observ.

OLD DRAWINGS OF COMETS

of 1848, but neither of the great comets of the nineteenth century was certainly identified with that of 1556, though there were great comets in 1843 and 1858. Most of the great historic comets, however, have been considered as unique appearances. Some have been visible close to the sun, some have had tails more than 90° in length, so that they appeared to stretch more than half-way across the sky. A few special ones must be noted besides those referred to, as probably observed at more than one perihelion passage. In B.C. 134, at the birth of Mithridates the Great, a comet, said to rival the brightness of the sun, was visible for ten weeks, its tail covering a fourth part of the sky and occupying four hours in rising. In A.D. 582 appeared a comet with a dark surrounding envelope, its appearance being likened to the smoke of a distant conflagration. In 615 is the first mention (from Chinese annals) of a comet that "wagged its tail." A very bright comet appeared in the spring of 1402, and another some weeks later, both of them said to have been visible in the day-time. Another, also visible in day-light, was observed in 1472 by Regiomontanus, from whose observations Halley computed an orbit, showing that it passed not much more than 3,000,000 miles from the earth. Another celebrated comet in 1618 had a tail more than 100° in extent, according to one observer; moreover, the tail wagged, or in the words of the observer, it was attended "*cum vibratione enormi.*" The great comet of 1680, with a tail like a scimitar, was observed by

Newton and Halley, and computed to have passed so close to the sun as almost to have grazed the surface. Halley tried to identify this with those of 1106, 531, and B.C. 43, since a period of 575 years would approximately fit them, but the observations themselves give possible periods varying from 805 years upwards, and Encke considered the probable period 8800 years, while the observations of Newton and Flamsteed give 3164 years, and the orbit may even have been hyperbolic, so that practically it is quite unknown.

Coming to the later years of the period under consideration we find two famous comets of the first half of the nineteenth century. The first was the great comet of 1811, which besides being very bright, was for a long time circumpolar, so that it was visible for many months. Argelander computed its period to be greater than 3000 years, so that its greatest distance from the sun worked out to more than 40,000,000,000 miles. Brighter still was the second great comet of the century, seen in 1843 at several southern stations in day-light. It had a long "wagging" tail, and in England, before news of its discovery arrived, people were puzzled by the appearance of some thirty odd degrees of tail after sunset, the head, which would at once have explained the phenomenon, having already set. This comet has been computed to pass within 100,000 miles of the surface of the sun, so that its perihelion distance is the smallest known, not even excepting that of the comet of 1680. The most important result of the appearance of this comet was the foundation of Harvard College Observatory.

# CHAPTER XIII

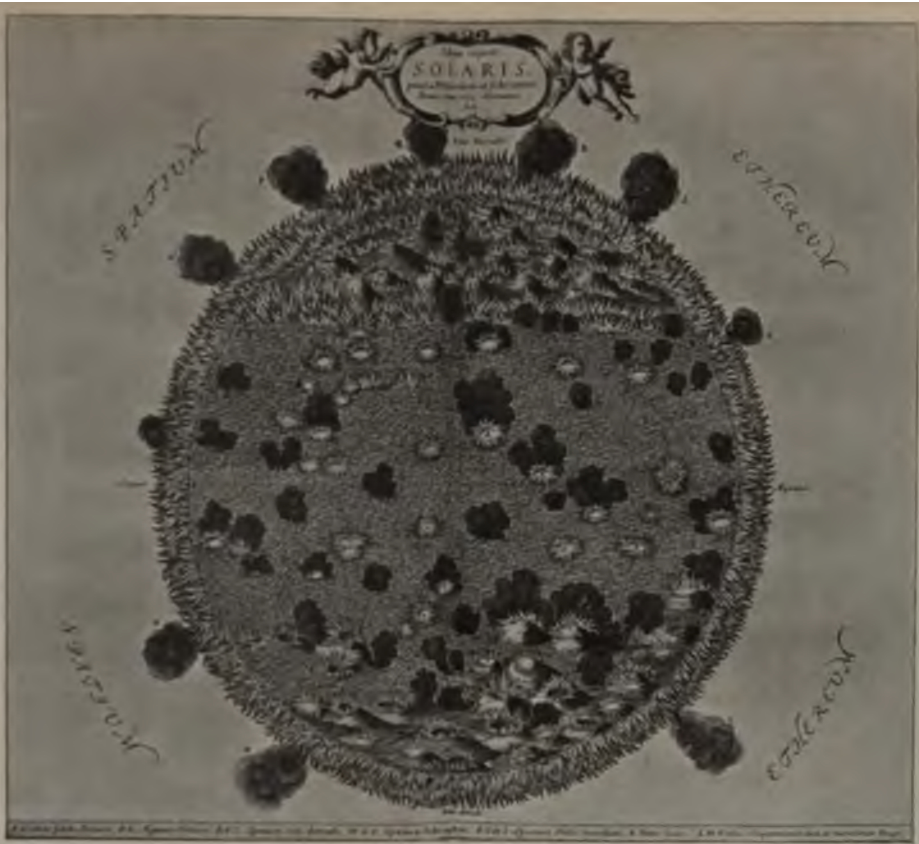
## THE SUN—ECLIPSES—PARALLAX

IT is time for us to leave the fascinating study of comets and consider the progress of solar astronomy. The sun is a body so vast that if it were in the position of the earth it would include the moon, although the latter is nearly a quarter of a million miles from the earth. The historic problem of the sun's distance reduces to that of finding a base line wherewith to compare it. Aristarchus chose the moon's distance, but failed because it was impossible for him to determine when the moon was exactly half full. Hipparchus attempted to work from the diameter of the earth's shadow as shown by the progress of a lunar eclipse. His result was no better than that of Aristarchus, both being twenty times too small, and no further progress whatever was made towards the solution until after the enunciation of Kepler's laws, showing the connection between the mean distances of the several planets. It was then seen that if the distance of Mars, for instance, could be determined, that of the sun would follow. From Tycho Brahe's observations Kepler found that the sun's distance could not be less than about three times the distance given by Ptolemy. Improvement came with the invention of the telescope, and Cassini's proposal, carried out by the Paris Academy of Sciences, was to compare the positions of Mars, as seen against the practically fixed background of stars, from distant stations in France and in Cayenne. What was strictly sought was not directly the actual distance of Mars, but the angle subtended at the distance of Mars by the earth's radius, from which the distance of Mars could be computed. This angle, however, was too small for exact measurement with the instruments then employed. But that very fact allowed Cassini to deduce that it could not have been greater than 25 seconds of arc, so that the corresponding angle (or parallax) of the sun could not exceed 10 seconds of arc, corresponding to a distance of more than 80,000,000 miles, a vast improvement on the previous best result. We have already referred to the attempt of Lacaille at the Cape, in conjunction with Lalande and others in Europe. The result, however, though definite and not conjectural, was practically the same.

But the transit of Venus affords another plan, and as one of these was to occur in 1761 and another in 1769, Halley proposed a method to be employed, depending on the fact that the duration of the transit of Venus across the solar disc would differ at different stations. If, then, the duration could be well observed at stations differing greatly in latitude, data would be provided for the solution of the problem. It would, however, be essential that both the beginning and end of the transit should be seen at each station, and since the transits take place either in June or in December, and may last eight hours, the weather conditions would be probably bad in the far south in the first case, or in the far north in the other, so that the choice of ideal stations would be restricted. Delisle's method, to obviate this, was to observe the exact time of one contact at stations differing widely in longitude, so that, instead of two chords of the sun of different length, whose distance from each other is measured by the difference of latitude on the earth, we have a direct comparison between an arc of longitude on the earth and the arc described by Venus in a known interval of time. This method necessitates extreme accuracy in the time determination, and in that of longitude, so that the weather conditions come in again with great effect. The results in 1761 and 1769 were a great advance on the previous ones; but though the latter in particular was well observed—a Government expedition under Captain Cook being sent specially at the request of the Royal Society, and many widely separated stations occupied by continental astronomers—the chief result was to demonstrate the great difficulty of the observation, which arises from the fact that owing to irradiation, the dark body of Venus, seen on the sun's disc, appears too small, so that it is nearly impossible to fix the time of contact. The peculiar difficulty involved is due to the gradual character of the effect, which only apparently reduces the part actually on the disc, so that the following limb of Venus appears distorted up to a certain, or rather uncertain point, when suddenly the round disc is seen at some distance from the limb of the sun.

As the next transit of Venus was only due in 1874, the requisite improvement in the accuracy of the determination had to be sought elsewhere, and the great advances in the precision of instruments enabled the sun's approximate distance to be deduced from its effect on the moon's motion. Laplace thus obtained a value practically equal to that which Encke found from a careful reconsideration of the transit of Venus observations taken in 1761 and 1769. The method is called that of the Parallactic Inequality of the Moon, and with improved observations and tables Hansen deduced a result in 1854 distinctly better than any previously obtained. Le Verrier obtained very nearly the same result by considering the apparent inequality of the sun's motion, due to the real inequality in that of the earth corresponding to that of the moon, inasmuch as both earth and moon really revolve about their common centre of gravity, though it is convenient to refer the whole of the motion to the moon. Another new method depended on the accurate determination of the velocity of light by actual experiment, from which the length of a chord of the earth's orbit could be deduced, by considering the retardation of the eclipses of Jupiter's satellites which, as we have seen, had been discovered by Roemer.

But before we pass to more modern advances, we must turn to other branches of solar research. Newton, and before him Grimaldi, discovered that sun-light was split up into different coloured rays when passed through a prism of glass, or any refracting substance. By varying the dispersion, Newton was unable to find any gaps in the coloured band, or spectrum; but in course of time, first Wollaston and then Fraunhofer discovered that there were breaks in the continuity, which could not be if light of every degree of refrangibility, (of every wave-length), were to be found there. Fraunhofer pursued the investigation with a telescope, and in 1814 mapped 576 dark lines. Space would fail us to trace the gradual emergence of the science of spectroscopy, of vast and increasing interest to the astronomer, but involving much work in the laboratory in which the chemist and the physicist claim their share. Herschel's chief contribution was the discrimination between heat-rays and light-rays, in which, after other physicists had found that the red end of the spectrum received more heat than the brightest portion, he made the further discovery of invisible heat-rays beyond the red end, among which he found a still greater thermal effect. We shall have occasion to review some of the recent developments in a later chapter, so now we turn to another interesting page in astronomical history, connected with the sun's surface. We have seen how among the many discoveries following inevitably from the invention of the telescope, that of sun-spots was made by Galileo and others, of whom, according to many authorities, Fabricius was really the earliest, though priority is also claimed for Galileo, Scheiner, and Harriot. The real credit lay with the telescope, so that the question of priority is of no importance. Considering how often sun-spots are visible to the naked eye, or rather without telescopic assistance, it would be difficult to believe that sun-spots had not been seen before, and as a matter of fact they had, centuries before, but it had never occurred to any one that they were sun-spots, and not simply bodies, such as Mercury for instance, passing between the earth and the sun. But, though Scheiner at first thought they might be planets revolving close to the sun, there was soon no doubt that not only were the spots solar but that they moved, showing that the sun was rotating in a period of twenty-seven to twenty-eight days. The results were viewed as heretical, partly because the venerated Aristotle said nothing about spots in the sun, and partly because it was considered derogatory to the sun to be other than perfectly bright. But even the bigotry that denied the truth of the Copernican system could not remain proof against the plain evidence of the senses, and soon the study of sun-spots was a recognised branch of astronomy. The bright spots were called "faculæ" by Hevelius, but observation was generally confined to the dark spots. Cassini described in great detail the variations in the appearance of some spots and groups of spots. The way in which gradations of light were shown between the spots and the bright part had been noted long before, and Cassini practically described the foreshortening effect shown by the spots and penumbra when near the sun's limb. It was, however, Dr Wilson of Glasgow who, in



AN OLD DRAWING OF THE SUN

1769, studying a large "naked-eye" spot, noticed that the penumbra near the limb looked different on the two sides of the spot, being wider on the side towards the limb, and almost, if not quite, vanishing on the side towards the centre of the sun. This could only mean, he considered, that the penumbra represented the sloping sides of a hollow in the place occupied by the spot, as, if the spot were level with the surface, there would be practically no relative foreshortening, and if raised the effect would be the opposite to that observed. Wilson saw that his hypothesis required to hold at both limbs, and so having seen the effect as the spot was going off the limb, he waited until it reappeared on the other side and found, as he expected, that the wide part of the penumbra was then on the opposite side, *i.e.* still towards the limb, while that towards the centre was out of sight. It is not by any means certain that the "Wilsonian theory," as it is called, is true; as the same effect might be produced in a different way, but it represents almost the only tentative advance in the subject until Herschel's time. Herschel worked and wrote much on the probable nature of the sun. To him it was a giant planet, the primary of our system, but otherwise very like any other planet, probably inhabited, inasmuch as he saw no reason to suppose the fiery heat we experience from the solar rays to have anything like a proportionate effect on the solid body he inferred to exist beneath the shining layer which we generally see. His scheme was simple and not lacking in plausibility. The sun like the earth is surrounded by an atmosphere partly transparent and partly charged with gaseous clouds: the outer layer incandescent, the inner opaque, reflecting light and heat from the outer. An uprush of gaseous matter from below, through the inner envelope, will just raise a sort of blister in the outer one (a facula technically), and if persistent, a sun-spot. Any gap in the outer envelope only will cause a dusky appearance, gaps in both occurring at the same place will cause the sun's dark body to be seen through, causing the appearance of a spot with a dusky penumbra, if the gap in the outer envelope is larger than that in the inner. Such was briefly what may be considered the first physical theory of the constitution of the sun. Herschel appealed to the fact of great cold experienced on mountain tops and high in the air as an argument that the sun's rays alone did not constitute heat, in answer to objectors to his theory of habitability. His eminent son seized upon another phenomenon first noted by Galileo: that the outbreaks of spots were almost entirely confined to two regions of the sun, zones of 35° on each side of the solar equator, separated by an equatorial zone of 16° breadth, in which, as well as in the circumpolar zones, very few have been certainly seen. He likened the equatorial zone to the zone of calms between the regions of the trade winds on the earth, and postulated some cause analogous to the circulation of air from the poles to the equator modified by rotation, which is the known cause of the terrestrial phenomena referred to. His suggestion for the purpose was a transparent atmosphere outside the luminous envelope, which by the rotation would be impelled to assume a form similar to that of the earth, flattened at the poles and bulging at the equator, and that this difference of thickness of atmosphere between poles and equator involved different rates of radiation, which would correspond to the difference of temperature on the earth and set up the circulation required. The analogy is very pretty, and the parallel between the monsoon and cyclone of our lower latitudes and the solar disturbances in his corresponding regions is striking, but it is after all only an ingenious hypothesis. The next advance was made by Schwabe of Dessau, who began noting the number of spots to be seen every day the sun was visible, apparently without any definite object, but was soon provided with one, for the percentages of days on which spots were visible in successive years, commencing with 1826, were: 93, 99, 100, 99, 82, 48, 56, 93, 100, 100, 100, 100. From this striking series of numbers, combined with certain other more or less definite signs of progressive change, which he did not fail to notice, such as the great rapidity of change half-way between the maximum and minimum, compared with the slow rate at those epochs, Schwabe felt confident that there was a true periodic variation in the phenomena. He continued his observations for twenty years longer in order to convince others, and ultimately succeeded in proving the existence of a period of about ten years. The completion of the analysis of his results must be deferred for the present, as he continued working well into the latter half of the century.

Before leaving the subject of the sun, however, we must pay some attention to the steady increase of knowledge due to the interest taken in solar eclipses. It must have been noticed many centuries before authentic records begin that there is something visible during a total eclipse, even of the longest possible duration, which prevents the darkness rivalling that of night, though even in more or less authentic accounts there is occasionally a doubt as to whether the description of a particular eclipse refers to an annular or total one. Since the moon and the sun, though apparently about equal in size, both vary their distance from the earth, it is obvious that the question of any totality of eclipse and of its duration depends on the relative distances of the sun and moon when in conjunction. If the moon is at apogee and the sun at perigee at a conjunction, the moon will be relatively small and the sun large, so that the moon will not entirely eclipse the sun, but will leave a bright ring or annulus, shining as clearly as usual. If, however, the moon is at perigee and the sun at apogee, it is the moon that is relatively large, and the eclipse will be total if the moon is near enough to the node of her orbit to render an eclipse possible (since no eclipse can occur unless the moon is in or near the ecliptic). In this case, also, there is during totality a bright ring visible round the moon, but of a totally different character, and called the "corona." The first mention of this is made by Philostratus in his "Life of Apollonius," the "prodigy," which was taken to foretell the death of Domitian, being referred to the year

A.D. 95. There had been historical eclipses before, mentioned by Herodotus and others, including the much-discussed "Eclipse of Thales" and "Eclipse of Agathocles." It is by means of these ancient eclipses that astronomy comes to the aid of history in helping to elucidate obscure points of chronology. It is, however, to be remarked that in more than one instance this aid is only partially successful, and leaves dates still uncertain, though the limits of uncertainty are narrowed. One great cause of this is the difficulty of deciding from the ancient record whether a given eclipse was or was not total, and this may very possibly be due to the brightness of the corona, though this is very inferior to that of the sun, or else it would be visible without an eclipse.

Naturally enough, for many centuries such phenomena were regarded as prodigies and omens, the few scientific observations being directed towards the determination of the moon's mean motion. Solar total eclipses, moreover, are rare, and especially so at any given place, none such having been visible in London, as Halley remarked, between 1140 and 1715; and it was not until nearly the latter of these dates, in 1706, that anything like systematic physical observations of the phenomenon were recorded. From that date onwards the corona was regularly observed, with notes as to stars and planets visible during the obscuration of the sun, and in 1733 we have the first mention of reddish spots near the moon's limb. In 1766 four extensions of the corona were remarked, giving it the familiar "oblong" shape. But the first occasion on which the astronomical world devoted its energies to the express purpose of making the most of the opportunity afforded was in 1842, though at the eclipse of 1733 a large number of the country clergy of Sweden responded to the suggestion of the Royal Society of that country, and made careful observations, which were collated and arranged by Celsius for publication in that Society's transactions.

The "rosy prominences" were again remarked in 1842, and discussion arose as to whether they belonged to the moon or to the sun. On this occasion, moreover, a distinction was made by some observers, notably Arago of the Paris Observatory, between the bright and comparatively narrow "inner corona" and the fainter extensions, partly radial and partly curved, forming the "outer corona"; but observers in general differed as to the exact form and extent of what they saw, probably in consequence of the varying atmospheric conditions at the several stations in France, Italy and Austria; especially as the sun's altitude varied from 12° at Perpignan to more than 40° at Lipesk, where Otto Struve remarked on the intense brilliancy of the corona as almost too bright for the unaided eye, while Arago at Perpignan compared it only to the light of the moon. "Shadow-bands" were also observed at this eclipse. A French observer, happening to look at a white wall at Perpignan as the last rays of the sun were disappearing, noticed a rapid undulation of the light on the wall, like that produced by the reflection of sunlight on the ceiling from the surface of liquid in motion, and remarked the same phenomenon at the end of the eclipse. Similar notes were made by others, both there and elsewhere. The celebrated "Baily's beads," however, were not seen on that occasion, though it was principally with the idea of confirming his previous observation made at the annular eclipse of 1836 that Baily went to Italy in 1842. He had, in 1836, noted that when about 90 per cent. of the moon's circumference had passed in front of the sun, the remaining arc suddenly took the appearance of a string of bright beads, irregular in size and distance, with intervening dark spaces, apparently glued to the sun's limb but gradually stretching away until suddenly the dark lines disappeared, and the moon appeared well inside the limb of the sun. This part of the phenomenon is obviously similar to the appearance already referred to in regard to the transit of Venus, the beads themselves being due to the irregularities of the moon's limb. Partial confirmation had been obtained in America at the eclipse of 1838, but practically none in 1842. In India, however, in 1847 Captain Jacob once more obtained partial confirmation of Baily's observation. The want of uniformity in the occurrence of the phenomenon is the principal stumbling block in the way of the general acceptance of the extremely plausible explanation that the cause is "irradiation," by which a dark body on a bright background appears smaller and a bright body on a dark background larger than it really is. Shadow-bands are often explained by a different optical principle, that of "interference," by virtue of which alternate bright and dark bands are produced by admitting sunlight through a narrow slit, the same being presumed to take place when the "slit" is personated by the narrowing portion of the uneclipsed sun.

In connection with the sun, reference has already been made to the transit of Venus,[1] but perhaps a little more attention is due to the first authentic observer of the phenomenon, *Venus in sole visa*, Jeremiah Horrocks or Horrox. Transits of Mercury had probably been seen before Kepler's time, but his Rudolphine tables gave the first known prediction of transits of Mercury and Venus, both of which, he announced, would cross the sun's disc in 1631—Mercury on November 7 and Venus on December 6. Gassendi at Paris observed the transit of Mercury, but was prevented by clouds from seeing that of Venus. Owing to an error in Kepler's tables, he announced that the next transit of Venus would not take place until 1761. Horrocks, however, discovered that the true place of the planet was between those given by Kepler's and Lansberg's tables, and that, though Kepler's made it pass below the sun in December 1639, Lansberg's gave a transit over the upper part of the disc. He concluded, therefore, too late to inform astronomers generally, who were relying on Kepler entirely, that Venus would actually cross the lower part of the sun's disc, and he and his friend Crabtree had the great satisfaction of verifying this, and

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[1] See p. 104.

Horrocks himself actually made observations during the transit. He was a young man of very great promise, whose very early death was a grievous loss to science. Dying in his twenty-first year, much of his writings being lost or destroyed in the Civil War, during which Crabtree also perished, there yet remains enough to secure him a high place among the successors of Hipparchus. His lunar theory supplies the explanation of the evection by a libratory motion of the apsides, and a variable eccentricity, and was probably of great value to Newton; the notion of a disturbing force of the sun, and what is often called Hooke's experiment, to show how apsides move, are due to Horrocks, who also realised the failure of a swinging body to represent the real facts, inasmuch as it moves about the centre instead of the focus of the elliptical path. He also worked during the last year of his life on the great inequality of Jupiter and Saturn, and projected investigations on comets and tides, and in the short time at his disposal, and with his very inadequate means, he made wonderful progress. In the early days of the Royal Society, when his papers were discovered and published, many must have echoed the cry of Dr Wallis, who edited the fragments, "Had he but lived, what would he not have done?"

# CHAPTER XIV

## GENERAL ASTRONOMY AND CELESTIAL MECHANICS

AS we come to more modern times, especially after Herschel's pioneer work had so greatly increased the number of astronomers, the biographical element becomes relatively of less importance, and it is necessary simply to follow in turn the development of the main branches of the subject, referring only incidentally to those whose labours are responsible for the development. Even so it is not easy to separate every branch of astronomy, since, to give only one instance, the subjects of instruments, of the sun, and of spectroscopy, are closely interwoven at several points. The difficulty is not a new one, and every attempt to evade it must be in the nature of a compromise. Following the main divisions of the "International Catalogue of Scientific Literature," the first group demanding attention is the general literature, history, and bibliography of the science, but it will be sufficient for our present purpose to treat this division with "considerable brevity." Text-books abound in many languages, from Herschel's "Outlines of Astronomy," which has been translated even into Chinese, to modern treatises, of which, whether in popular form or for the use of students, the number is very large; those of Chambers and Flammarion, for instance, belonging more to the former category, while leading examples of the latter are due to Professors Chauvenet of St. Louis and Young of Princeton. The history of the science has by no means been neglected; the great work of Delambre, himself an astronomer, still remains a veritable treasure-house, though for more modern research (the date of Delambre's last volume is 1821) Wolf's "Geschichte der Astronomie" must not be overlooked by the biographer; and the development of the science itself may be studied in parts of Whewell's "History of the Inductive Sciences," or with great wealth of detail in Professor Grant's "History of Physical Astronomy." Besides these and many other treatises, the progress of astronomy has been reported regularly in the Monthly Notices of the Royal Astronomical Society, and in numerous other publications, the most important of which are, perhaps, Airy's report to the British Association in 1832, those of Professor Loomis in America, and of the Smithsonian and kindred institutions. We have already referred to the foundation of some of the journals or publications devoted to astronomy, and to the societies whose interest lies entirely with the science; and progress in that direction also has been very rapid since the middle of the last century. The German Astronomical Society was founded in 1860, that of France in 1887, the British Astronomical Association, which has Colonial branches, in 1890; and though, owing to the distances, it has been found almost impossible to work a society for the

United States, large sections of it are not unprovided with such advantages; as, for instance, the Astronomical Society of the Pacific, founded in 1887; while over the northern boundary flourishes the Astronomical Society of Canada. All these and many others, including local societies such as those of Liverpool, Leeds and Montpellier, publish monthly journals or annual reports; and, in addition, there is an increasing number of more or less independent periodicals. The American "Astronomical Journal" was founded by Gould in 1851, and though the Civil War, followed by Gould's absence in the Argentine, caused a break of a quarter of a century, it revived in 1888, and is still in the front rank. The "Bulletin Astronomique de France," under the ægis of the Paris Observatory, appeared first in 1884, six years after the first issue of "The Observatory," which has always been closely associated with Greenwich, and which has practically superseded the "Astronomical Register" (1864-1886). Another American journal, "The Sidereal Messenger," founded in 1883, enlarged and renamed "Astronomy and Astrophysics" in 1892, on account of the great advances being made in spectroscopy, was succeeded by the "Astrophysical Journal" in 1895, the more popular part of it being continued from 1894 as "Popular Astronomy." Another form of publication, the various series of bulletins and circulars of different observatories, must also be noted: among older series were Brünnow's "Astronomical Notices" from Ann Arbor, and the "Dun Echt" and afterwards "Edinburgh" circulars from

Lord Crawford's Observatory; but the most important ones at the present day are those of the great American observatories, such as the Lick Observatory bulletins, the Yerkes Observatory bulletins, and the Harvard Observatory circulars. Some of the publications in the above list are not altogether confined to astronomy, as in observatory publications, for instance, space has often to be found for meteorology; but, on the other hand, large numbers of scientific journals find space for a certain amount of astronomy, either in the form of extracts from astronomical journals, or occasionally in specially contributed articles.

One other special set of annuals belongs also to this section, viz., the various national ephemerides, of which the most important are the Nautical Almanac, the Connaissance des Temps, the Berliner Astronomische Jahrbuch, and the Washington Ephemeris. Formerly many important papers were included as appendices in some of these almanacs, notably the Connaissance des Temps; but the increasing facilities for publication elsewhere have practically put an end to the practice; nothing, for instance, of this character appearing in any more recent number of the Connaissance than 1878.

The problems of spherical astronomy are of great antiquity; and as in this branch of the science the exact causes of the celestial motions were not of vital importance, not so much is left for modern research, though advances in methods of computing are continually being made, principally in the direction of shortening labour. The theoretical determination of the latitude and longitude, the essential problem of nautical astronomy, has always been of great importance; and it is almost inconceivable that any advance of human knowledge will minimise the necessity for the study of this problem, though with the increasing reliability of chronometers many of the older methods of determining longitude have ceased to have much practical value. It is just possible that some development of wireless telegraphy may enable a ship's position to be sent by return ethergram from the nearest land station, but in no other direction does there seem to be the slightest hope. The ordinary problems of the conversion of altitude and azimuth into right ascension and declination, of the time of rising and setting of the sun, moon, or any other celestial body, have long ceased to present any new features. The corrections to reduce observations to the centre of the earth, and to allow for the effect of the earth's various periodic motions, in fact to reduce to the centre of the sun, form parts of the same domain; and so also do the calculation of ephemerides, including eclipses, occultations, and what are called planetary phenomena, such as conjunctions of planets with each other, or with the moon or a conspicuous star, and eclipses, occultations and transits of Jupiter's satellites. In some of these directions advances have been made in the way of graphic or mechanical methods of computation; in others the improvement lies rather in the more accurate discrimination between small quantities that may safely be ignored, and others which would produce cumulative errors. A new work on spherical astronomy by Professor Newcomb is typical of this form of economy of labour, and is a notable contribution to the subject.

Dynamical astronomy, as we have seen, is a much more recent product, and may be said to have owed its birth to Newton, and its vigorous development to the great continental mathematicians and physicists to whom reference has already been made. It is true that from time to time fresh or apparently fresh theories are produced, either to sweep away the remaining uncertainties caused by observed motions for which the Newtonian theory has hitherto failed to account, or to modify the law of gravitation itself in order to compromise with those discordances. The great historic instance is the motion of the perihelion of Mercury, an example of the first class of theories being the assumption of a resisting medium, effective only within a comparatively small distance from the sun; an assumption which was supposed to be capable of simple test by means of Encke's comet, which also has a small perihelion distance; an example of the second class being a totally different assumption, that the law of gravitation is not strictly true, there being a supposed small additional term depending upon an inverse power of the distance of a higher order than the square, so that it would have more effect near the sun. Neither of these assumptions can be said to have been justified by observation, and the slight discrepancy which called them forth still remains. But an investigation into the long and valuable series of Greenwich lunar observations which has quite recently been carried out by P. H. Cowell at the Royal Observatory, has caused it to be regarded as at all events possible that salvation may lie in a more accurate adjustment of the known facts, without any gratuitous assumptions whatever.

As to the mode of action of the universal force of gravitation, a time element has been sought, as it seems incredible that the action should be instantaneous at all distances. Professor Whittaker, the newly elected Royal Astronomer for Ireland, has suggested an undulatory theory of gravitation, based upon a solution in general terms of Laplace's fundamental equations; but physicists deny the validity of his explanation.

In the calculation of orbits from a limited number of observations, progress has been perhaps more quantitative than qualitative, the increasing number of "new" planets and comets affording plenty of scope for the diligent computer; while the theoretical advance has been to a large extent directed towards the restricted problem of three bodies in which the motions of two of the bodies are nearly commensurable; for instance the case of minor planets whose mean motion is twice that of Jupiter. Much work has been done, especially in Germany in recent years, on the general theory of perturbations, and on various subsidiary problems that arise in the course of such investigations; but probably the most regular advance has been made in the lunar theory. At the beginning of the nineteenth century the secular acceleration of the moon's mean motion had already been investigated by Laplace, and early in the century his value for it was approximately confirmed by Damoiseau and Plana, and again before the middle of the century by Hansen. But soon afterwards Adams took up the question, and found an error which had had the effect of doubling the real value. Hansen, however, in his lunar tables, published in 1857, ignored Adams's result, and used a slightly larger value than before. Delaunay thereupon attacked the question and confirmed Adams's smaller value, which was ultimately accepted. The older value, however, seemed to satisfy the records of ancient eclipses better than the new one, which, therefore, left something further to be accounted for. It is possible that Cowell's researches into the subject of ancient eclipses may supply the explanation, as his conclusions suggest that there is probably an unsuspected acceleration of the sun, *i.e.* of the earth's mean motion also, and that allowing for this the inconsistency disappears.

Airy worked for years on a numerical lunar theory using the long series of Greenwich observations, but was not himself satisfied with the work. Newcomb published corrections to Hansen's tables and contributed also to the theory. G. W. Hill, another American mathematician, attacked the whole lunar theory in a different manner, and also continued the work left unfinished by Delaunay. For his "epoch-making" researches Hill was awarded the Astronomical Society's Gold Medal in 1887, similar awards for lunar researches having been made to Damoiseau, Plana, Hansen, Delaunay and Adams. The latest award, in 1907, was also in connection with the same subject to Professor E. W. Brown, who has for many years been working at Haverford College on the lines laid down by G. W. Hill, evaluating coefficients for nearly four hundred periodic terms (Euler used only thirty), involving millions of figures, and checking every step by independent equations of verification, ensuring an accuracy probably far in advance of that of previous workers. His work is to be continued at Yale, and he hopes to complete the coefficients of all the inequalities amounting to a hundredth of a second of arc, and to construct new and more accurate lunar tables.

Research on similar lines, into which we cannot enter in detail, has also been continued in planetary theory, and in more accurately evaluating the more obscure inequalities due to their mutual action; and it is, as before hinted, in this direction that the ultimate vindication of the Newtonian theory may be found.

Very valuable work has also been done, and is still being attempted, on the theoretical "figure" of celestial bodies. Professor Darwin in England and Poincaré in France have investigated such abstruse subjects as the effect of tidal friction in determining the birth and subsequent career of a satellite, and on the stability of various forms possible for a rotating mass of fluid such as has been conjectured to have been the original state of all planets, the weight of evidence in favour of the pear-shaped form having recently had an accession in consequence of earthquake investigations, coupled with the great preponderance of land north of the equator. The figure of comets has been another fruitful field for more or less speculative theory, much work being due to the late Russian Professor Bredikhine, whose interest in the subject is commemorated by a prize periodically awarded for cometary investigations similar to his own. In this connection much has been made of the theory, generally attributed to Professor Arrhenius, of the repulsive action of light on small particles; small, that is to say, even in comparison with atoms, such as are supposed to form the tails of comets; this is also called the pressure due to radiation, and it has been suggested that radium or radio-activity is the key to many of the riddles of the universe. Cosmogony itself has not been forgotten. Kant's cosmogony, or practically Laplace's nebular hypothesis, is not by any means alone in the field. Lockyer's meteoritic hypothesis substitutes for condensation from one nebula into a central body surrounded by revolving satellites, with or without their own companions, the alternative of a gradual building up of the system by successive collisions and interferences between swarms of meteors more or less pervading space. The most recent analysis of tidal action and the forces acting on swarms of particles have gradually given rise to a new "hypothesis," to which the name of "planetesimal" has been given, one essential difference being that it does not start with an assumption, however plausible, as to what might have been the original condition of things; but reasons from analogy with a state which is exceedingly common, if not universal, in the nebulæ, an aggregation of particles in the form of a spiral.

In previous hypotheses Saturn's ring has been regarded as a stage of development through and beyond which all the other satellites and planets in the solar system have already progressed. Professor Darwin and others have given a fairly conclusive denial to the possibility of a ring condensing at any point other than the centre. The new cosmogony regards Saturn's ring as the necessary ultimate form of a satellite revolving too near its primary to retain stability under the enormous tidal action, and hence breaking up into particles scattered round its original orbit. In this direction and in others, notably the anomalous cases of the outermost planets and certain satellites, the weight of evidence has steadily accumulated in favour of the planetesimal hypothesis, a large share in the development of which has been taken by Americans, among whom the names of Moulton, Stockwell and Chamberlin are conspicuous in this investigation.

One phase of the subject of the universe and stellar systems is the ever-recurring question of a medium, or ether of some kind, pervading the spaces between the celestial bodies. It has been argued that if space is limitless, and stellar systems scattered in every part of it, it might be expected that in every direction some star would lie, and the aggregate effect of so many infinitesimal points would be a continuous lighting up of the whole sky. Since this is demonstrably not the case, it has been suggested that either space, or, at any rate, interstellar space, is not limitless, or else that the hypothetical "ether" does act as an absorbent at vast distances, and practically extinguishes the light of every star fainter than a certain limit. There is not much to be said on this subject, to which, however, we may recur later on. Another matter which does not strictly come in this connection, perhaps, is the motion of the solar system in space. Since Herschel's time many investigators have attacked this problem in more than one direction, using various material. It was once regarded as almost an axiom that faint stars were more distant than bright ones, but a very little thought shows that this is only another way of saying that all stars are actually of equal brightness, which is, in the first place, hopelessly improbable, and, in the second place, absolutely contrary to observed fact. The history of the investigation is a capital instance of the epoch-making results that can arise from the careful pursuit of a definite object even upon what may be termed false lines. Herschel's endeavours, on the above erroneous assumption, to determine the actual motion of a bright star, or of the solar system with respect to the bright star, or the distance of the star, by a series of measures of direction and distance of a neighbouring faint star, revealed in many cases an actual motion of one star relative to the other, and laid the foundation of double star astronomy. He did nevertheless find sufficient evidence of systematic proper motions to assign a position to the "solar apex," or point in the heavens towards which the solar system appears to be moving, but later researches have considerably modified his result. It may be stated, with sufficient accuracy for our purpose, that the solar apex is "not far" from the direction of the bright star Vega in the constellation of Lyra, but a few remarks are necessary as to the uncertainties of the problem. The so-called proper motion of a star is a very small quantity, and is made up of the apparent effect of the real motion of the solar system and of the actual motion of the star itself. One or both of these in certain directions will be zero, but in general both are present; and so, however many stars are taken, and however great the accuracy of the measures, there must be a slight want of determinateness in the result, as the number of unknown quantities is always one more than the number of equations. However, since it is exceedingly probable that the motions of the stars, whether systematic or arbitrary, will tend to balance each other when a large number is taken in all parts of the sky, there appears an obvious way out of the difficulty. We are now face to face with another, for the number of stars whose proper motions can be regarded as approximately known is not large, nor are they evenly distributed.

If, in order to increase the numbers, we include very small proper motions, then the probable error of the determinations becomes relatively too large for any confidence to be placed in the result. Different investigators have chosen different modes of compromise with these conditions, and the net result has been that according as the principle of selection varies, so will the apparent position of the solar apex. Bright stars give one value and faint stars give another; stars of one type, so far as revealed by the spectroscope, give one result, stars of another type, a different one. The stars which have the largest proper motions have usually the largest parallax on the whole, so that, being relatively much nearer, the effect of their actual motion is greater, the only actual gain being in the relatively less importance of errors of observation. Attempts have been made, especially by Professor Kapteyn of Groningen, to determine the result by taking different regions of the sky each as a whole, but there is much the same uncertainty about the final figures; so much so that it has been suggested with some plausibility that there are at least two systems in the universe moving in different ways, and that the problem cannot approach solution until these can be separated. It is in this direction that much is hoped from the spectroscopic method, on the rather arbitrary assumption that stars of one type are more likely to belong to one system.

# CHAPTER XV

## OBSERVATORIES AND INSTRUMENTS

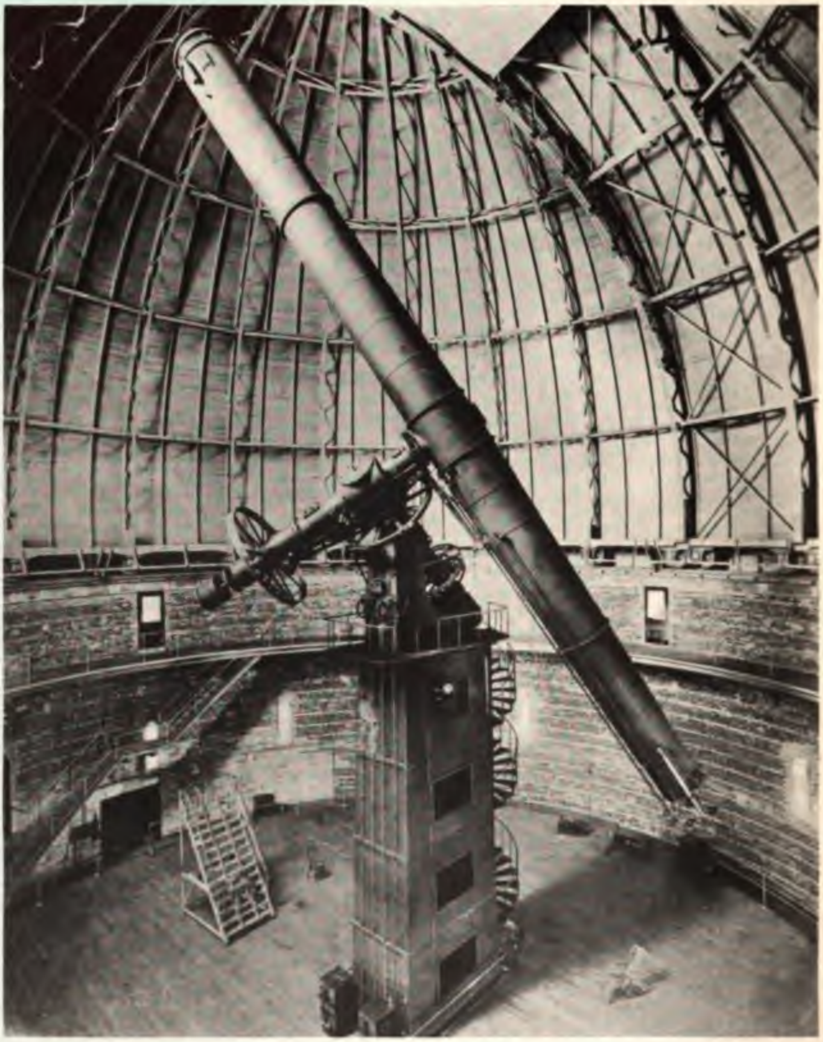
THE next division of the subject is that of observatories and instruments. We have noted the foundation of the early national observatories, and in later times the impetus given to private effort by discoveries in the heavens, in particular those of Herschel in descriptive astronomy. Astronomy is so obviously a practical application of mathematics that it is bound to receive recognition at most universities, where private munificence has in general been the only source available for providing the necessary buildings and instruments. It is to this source that much of the progress in America may be attributed. In the States observatories are numbered by hundreds, many attached to universities and colleges, but known almost better in some cases by the name of the founder. We may instance as those most widely known the Lick Observatory of the University of California at Mount Hamilton, and the Yerkes Observatory of the University of Chicago at Williams Bay, Wisconsin. Harvard College Observatory is a partial exception, inasmuch as Harvard founded the whole, and not the observatory only; and there are other examples. The U.S. Naval Observatory at Washington D.C.

was organised in 1842, though observations had been taken at a temporary observatory for some few years previously. Since 1861 valuable work has been continuously done there; though until 1893, when a new observatory was opened in a better position, the situation of the Naval Observatory was too near the Potomac River. In the near neighbourhood was the first Astrophysical Observatory of the Smithsonian Institution, and not far away the Jesuit College of Georgetown has an observatory founded also in 1842.

Of the many observatories attached to American colleges, Harvard, though until recently far in advance of any other, is not the oldest, since it dates back only to 1839, whereas Yale had an observatory as long ago as 1830. Princeton has two, much more recent; and Chicago, before the opening of the Yerkes Observatory in 1897, was provided with the Dearborn, built by subscription in 1862, and also the Kenwood Observatory. The first observatory founded by public subscription in the United States was at Cincinnati in 1842, removed in 1873 to Mount Lookout. It is consistent with American methods to move an observatory as soon as its position is proved to be suffering from climatic conditions or interfered with by buildings, smoke, or railway vibrations, and it is being continually preached by their men of science that it is worth while to spend much time and money in choosing the location for an observatory. Yerkes Observatory, for instance, is some eighty miles distant from Chicago. Professor Lowell has at last settled down at Flagstaff, Arizona, after trying to find an ideal spot; and quite recently the Smithsonian Institution, finding the neighbourhood of Washington unsuited for some of its purposes, has founded another Astrophysical Observatory at Mount Wilson, California, more especially for solar physics.

We shall have occasion to refer to many of the observatories in connection with the work done, so we must not attempt a full account of them. It must not be supposed that the United States stands alone in multiplying observatories, though its vast extent and the wealth and energy of its people have rendered it conspicuous in that way. The British and European Universities have also their share of observatories: Oxford, Cambridge, Dublin, Durham, Glasgow, Bonn, Göttingen, Königsberg, Leipsic, Strasburg, Prague, Cracow, Coimbra, Turin, Padua, Madrid, Bologna, Helsingfors, Dorpat, Warsaw, Moscow, Kasan, Upsala, Lund, Christiania, Leyden, Utrecht, and many others are equipped in greater or less degree, and there are many Royal Observatories in addition to Greenwich, such as Edinburgh, Berlin, Kiel, Munich, Vienna, Buda-Pesth, Madrid, Lisbon, Naples, Palermo and Brussels. Europe can also show many public and private observatories that do not come under either of these classes, some naval observatories and some geodetic.

There are also observatories in Africa besides the Royal Observatory at Cape Town, at Durban, Algiers, and a recent foundation at Helwan, in Egypt. India and Australia are also doing good



YERKES' 40-INCH EQUATORIAL

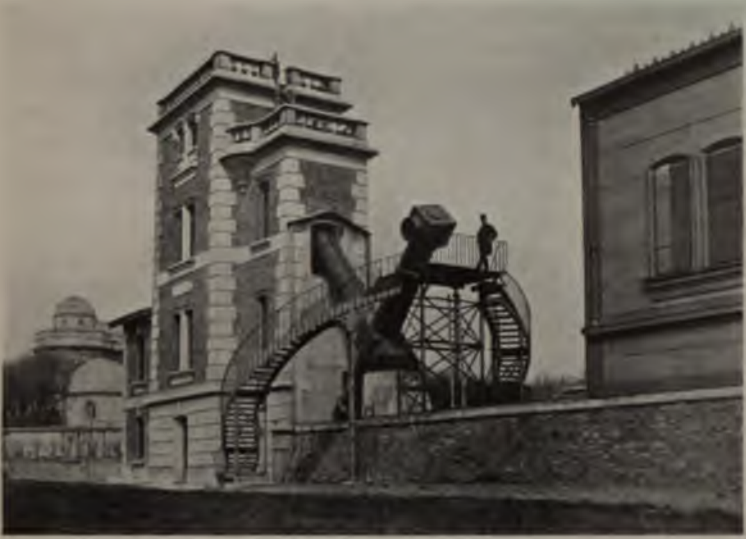
work, at Madras for instance, Sydney, Melbourne, and Perth, and there is a flourishing observatory at Tokyo University, Japan.

Private observatories, not connected with any university or institution, have also steadily increased in numbers, although in many notable cases the death of the founder means the removal or discontinuance of the observatory. For instance, the instruments of Baron d'Engelhardt, late of Dresden, form the principal equipment of the Kasan Observatory, while no one now works on the sites occupied by Herschel or Lassell, or to mention a recent loss, Dr Isaac Roberts.

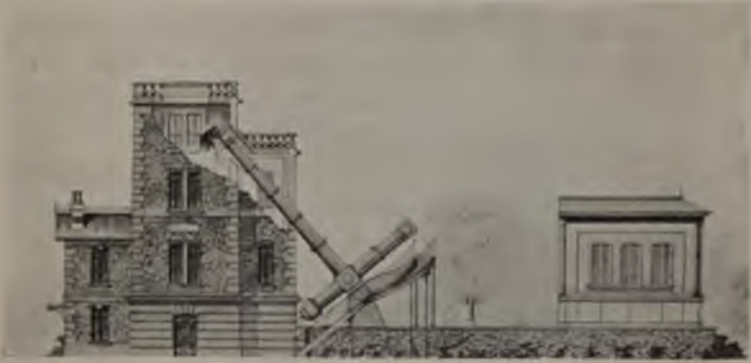
Few observatory buildings are on the scale of lavish magnificence of Pulkowa, but although perhaps more regard is paid to the practical details of pier-foundations, ventilation, ease of manipulation of domes, and such matters than to the purely architectural point of view, it cannot be said that modern buildings compare unfavourably even in appearance with older ones. In equipment they are manifestly superior. We have seen how the Königsberg heliometer yielded pride of place to the great Pulkowa refractor; but the advance in light-grasping power since Struve's time has been very great, and at times exceedingly rapid.

The 40-inch refractor of the Yerkes Observatory represents the most powerful instrument of its kind, its successful completion in 1897 having ended the nine years' supremacy of the Lick 36-inch refractor. Other large telescopes of the refracting type are the 33-inch at Meudon (Paris), 31½-inch at Potsdam, 30-inch at Nice, 30-inch at Pulkowa, 29-inch at Paris, 28-inch at Greenwich, 27-inch at Vienna, 26-inch at Greenwich, 26-inch at Washington, 26-inch at University of Virginia, and the 25-inch, the first of the large refractors, constructed in 1870 for R. S. Newall of Gateshead, and since presented to Cambridge University, where his son, H. F. Newall, is working with it. The Paris Exhibition of 1900 was equipped with a refractor theoretically far in advance of any of these with an aperture of 49 inches, but it could not be used as an ordinary equatorial owing to the great weight that would have to be moved and the consequent strain of its parts, so that it was arranged in a horizontal position in conjunction with a large plane reflector whose motion should allow any desired object to be seen. The instrument was not a great success, wherein the predictions of astronomers were verified.

The greatest recent French invention in connection with refractors has to do not with the size but with the mounting. Loewy's Equatorial Coudé is now the standard type for French equatorials, and has been recently introduced into Cambridge University. It has the great advantage that the observer need not move at all, a result achieved by two reflectors which bring rays from any desired region in space to a direction parallel to the main axis of the instrument. Refractors are not the only telescopes, however, nor equatorials the only form of mounting. No telescope approaches in actual size the great Parsonstown reflector, of 6 feet aperture, erected by Lord Rosse in 1845, but one



COUDÉ EQUATORIAL. PARIS



SECTIONAL VIEW

of 5 feet has recently been provided for Harvard College Observatory. And though for many purposes, notably the discovery of faint nebulæ, the reflector has an advantage over the refractor, since so much light is absorbed in the latter in transmission through the object-glass, the balance of opinion in general inclines to the refractor as on the whole the more effective form, though most large observatories are equipped with reflectors for special purposes, mainly photographic.

In regard to instruments for fundamental purposes, rather than for the physical and differential observations for which an equatorial is suited, there have been equally great advances. In size it is considered the limit of expediency has already been reached, if not passed. Since the mounting of the Greenwich transit-circle, aperture rather more than 8 inches, in 1850, very few meridian instruments with apertures greater than 6 inches have been constructed. This is, however, largely due to the consideration that fundamental catalogues are not produced by many observatories, and that for the simple purpose of time-determination large apertures are unnecessary; and it is also maintained by some authorities that the great essential for fundamental observations of great accuracy is an instrument strictly reversible in all its parts, so that great size involving great weight is a distinct drawback. Without going deeply into this controversial subject it may be briefly noted that instruments cannot be constructed free from the possibility of residual errors of adjustment, for which observations must be corrected. The point at issue is whether this should be done by increasing the stability of the instrument and determining the actual amount of the residual errors, on the assumption that under such conditions they are not liable to capricious alteration; or whether many of the corrections should be rendered apparently unnecessary by adopting the reversible instrument and taking all observations in both positions, on the assumption that all the sources of error which change sign on reversing will disappear in the mean result. It is unsafe to dogmatise, but it can hardly be denied that in each case the assumption, though plausible, is not always justified, so that the question remains undecided. The great series of Greenwich catalogues and many others are obtained with non-reversible instruments, but an attempt is being made at the other Admiralty Observatory at the Cape of Good Hope to justify the reversible transit instrument on a large scale with highly elaborate precautions to ensure symmetry not only in the telescope itself, but in the observing room, the shutter openings and other arrangements being designed to reduce errors which half a century ago were either not thought of or deemed insignificant in comparison with the probable error of an observation.

Of instruments similar to the transit circle may be mentioned the transit in the prime vertical with axis north and south instead of east and west, of which the most important was established at Pulkowa; and the modern altazimuth, which is really a reversible transit circle, which can be used in the meridian or the prime vertical or any intermediate azimuth. Of instruments for special purposes there have been several from time to time. The Greenwich water telescope was constructed for the express purpose of testing a theory that aberration was modified by refraction through an absorbing medium such as a lens. Having conclusively decided the question in the negative, the use of the instrument was discontinued. The Reflex Zenith Tube was invented to determine by observation of $\gamma$ Draconis, the same star used by Bradley for the same purpose, the constants of aberration and precession, and incidentally the parallax of the star. Observations were gradually discontinued as they resulted in an impossible negative parallax, but since the discovery of latitude variation, which accounted for the anomaly, they have once more formed part of the routine at Greenwich, other stars close to the zenith being observed in addition to $\gamma$ Draconis.

For some purposes an instrument revolving about a vertical axis is desirable, but owing to mechanical difficulties the simplest way of obtaining this without having errors continuously varying from one azimuth to another, as they might be expected to do with the universal transit circle or modern altazimuth, has been found in the Almucantar, floating in mercury, a modern instance of which may be found at Durham Observatory. Other forms of telescope mounting may have to be referred to under other sections, but we must pass on to the next item in the schedule, noting by the way that the increased effectiveness of the modern instrument is largely due to scientific improvements in the manufacture of glass, at Jena in Saxony, and elsewhere.

A great saving of time in meridian observing was made by the American invention of the galvanic chronograph, so that the observer, instead of entering in his book the time to decimals of a second at which a star passed each wire in his instrument, records the instant electrically, so that the times can be read off at leisure instead of using the precious moments of a fine night for such mechanical operations. There are various forms of chronograph, in most of which the seconds or alternate seconds from a standard clock form points of reference either by pricking the paper on a revolving drum, or by marking it with dots, or by regular deflections of an otherwise continuous trace of a pen, the observer's signals giving intermediate points or deflections. There is also a printing chronograph invented by another American professor, G. W. Hough. Many developments are due to the advance in astronomical photography and in spectroscopy, but we cannot deal with them here.

# CHAPTER XVI

## ADJUSTMENT OF OBSERVATIONS. PERSONAL ERRORS

OMITTING all detailed reference to modified methods of observation and the reduction and correction of the results, there remain a few matters of interest before we proceed to the more striking results of practical observation of the heavenly bodies.

One of these is personal equation. Maskelyne, finding one of his assistants continually differing from him in estimating the time of a transit, discharged the man as incompetent, thus failing to appreciate the true inwardness of the fact he had discovered. The enormous difference of a second and a quarter between the determinations of Bessel and Argelander, both practised observers, was the cause of the discovery of the true nature of the discordance, though it is generally admitted that part of this abnormal discordance must be attributed to an absolute difference of reading the chronometer itself. (Chronometers in general beat half-seconds, and there is reason to assume that one of the distinguished astronomers was in the habit of counting his seconds on the wrong alternate half-beats.) The difference between two observers is a real thing, and appears to be a psychological phenomenon depending on the relative rate of perception of different senses. In the old-fashioned method of observing by "eye and ear," anyone whose hearing is quicker than his sight will record observations late. In the galvanic method the practice is not uniform: some proceed to make contact when they see a star bisected by a wire, being thus always late by intervals depending on their quickness of sight and touch; others endeavour to "shoot the flying bird," that is, to make their perception of the tap coincide with their perception of the bisection; this method, as a rule, comes perhaps nearer the truth, but is objected to by many on the ground that it is not so consistent, especially for stars of different declination, involving different apparent velocity over the wires.

Many attempts have been made from time to time to determine absolute personal equations; none, however, with conspicuous success, a weakness of some of the methods being the irregular oxidising action of galvanic contacts. More recently, however, a device has been produced by Messrs Repsold, of Munich, to abolish the "tapping" altogether, by means of a special micrometer, the observer keeping the star image bisected by a movable wire, which records its own revolutions automatically. With this very consistent results have been obtained, and the greater part of the difference of personal equation eliminated; if anything remains in the way of bisection error, it is sought to eliminate this by reversing the direction of motion for half of the observation. The longitude observations made with instruments of this kind by Professor Albrecht and others are a strong testimony to the efficiency of the device. A later refinement seeks, by an adjustable clock motion, to render the observation still more automatic, but is not yet perfected.

Personal equation is, unfortunately, not constant in general, but varies with the conditions. Small variations have been attributed to the observer's attitude, to the time of day, in reference to meals and to fatigue, to the time of year, in reference to holidays in their effect on general health, and, with great justice, to the magnitude of the stars observed. Magnitude equation, as it is called, is a peculiar form of bisection error, and it is generally found that observers are later for fainter stars. It would certainly seem worth while to eliminate so variable a quantity if it were practicable.

Another form of personal error is called decimal equation. In taking observations to estimated tenths of any unit, every figure from 0 to 9 ought to occur with equal frequency in a large number of observations; but it is generally found to be not the case, some observers having too great a proportion of 0 and 5, for instance, while the digits set down by others have quite a different order of frequency.

The determination of astronomical constants perhaps hardly belongs to this chapter, but it will be convenient to refer to it here. The principal constants in question are those of aberration, of precession and nutation (involving solar motion), and of refraction. There are other "constants" connected with every separate planet and satellite, but these are generally discussed under the planets concerned.

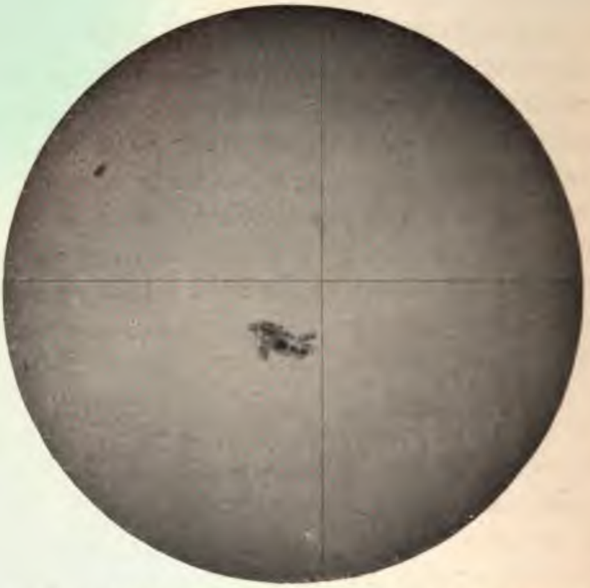
The Paris conference on fundamental stars, 1896, adopted values for the constants of aberration, precession and nutation, in order to secure uniformity, Professor Newcomb's exhaustive comparison of large numbers of modern observations having given results somewhat different to those of Struve for aberration, and of Peters for precession and nutation, previously in general use. Refinements of observation, rendered possible by special instruments and methods, some of which have been referred to in the previous chapter, tend to show that greater accuracy is not unattainable, and some observatories devote more time and energy to these general problems than to the possibly more attractive branches in which the majority more or less specialise.

Refraction is on a different footing, and there seems little hope of obtaining perfect tables for low altitudes. Observations of stars above and below the pole at the same observatory, especially of stars which pass near the zenith, coupled with observations of stars at two or more observatories in widely different latitudes, would seem to supply ample information from which to deduce the law or laws of refraction. But the practical impossibility of allowing for the varying amount, density, temperature, and other circumstances of the columns of air traversed by rays at low altitudes greatly discourages special observations for the purpose, and it is more usual to collect large quantities of observations which were not taken specially, such as those of stars common to catalogues of the same epoch obtained in different latitudes. It is quite possible that refraction tables which are satisfactory for one place will not be equally so for another, so that it might be well if more attention were devoted to this subject in fundamental observatories.

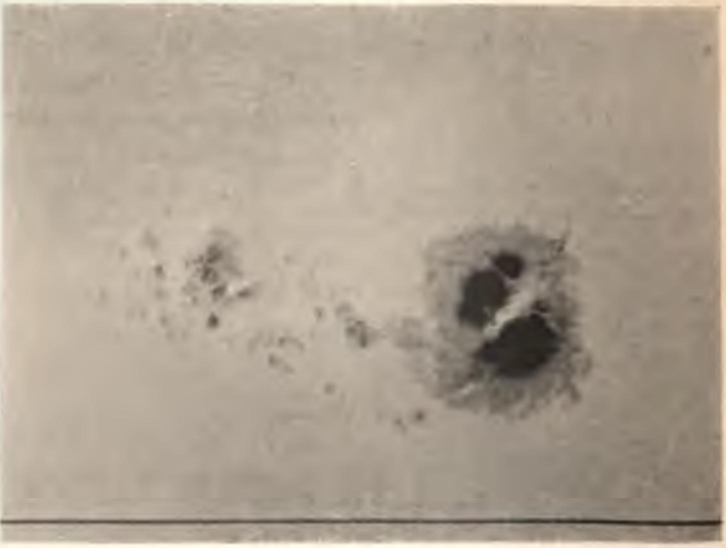
# CHAPTER XVII

## THE SUN

WE must now take up the threads laid down in Chapter XIII., and trace the progress made in solar astronomy since the middle of the last century. We have noted Schwabe's discovery of the "periodicity of sun-spots," as it is generally called, the actual state of the case being that the mean spotted area of the sun varies more or less regularly, increasing to a maximum and then decreasing to a minimum, the whole series of changes taking place in a period of approximately eleven years. It was a coincidence that not long after Schwabe commenced his investigation, the problem of terrestrial magnetism was also resolutely attacked, upon the original initiative of Humboldt, but under the direction of Gauss to a great extent. By 1841 magnetic observatories had been started at Greenwich, on the Continent, and in distant parts of the world. In 1851 Lamont of Munich discovered a period of between ten and eleven years in the amplitude of the daily variation of magnetic declination, and in 1852 Sabine found practically the same period for the frequency of magnetic storms, noting that this period agreed both in duration and phase with Schwabe's sun-



SUN, SHOWING LARGEST SPOT EVER PHOTOGRAPHED, 1905, FEBRUARY 4



ENLARGEMENT OF SUN SPOT, 1905, JULY 15

spot period. More accurate determinations from a greater mass of observations have emphasised this agreement, hinting at an intimate connection between sun-spots and terrestrial magnetism. The eleven-year period was tentatively applied to meteorological records with very little success, except in regard to the frequency of manifestations of Aurora Borealis, which showed a marked agreement with magnetic disturbances. As an example of the kind of result obtained in meteorological records we may instance the hot summers of 1846, 1857 and 1868, followed by the well-remembered 1879 with practically no summer at all. But "that is another story." Investigation into the connection between sun-spots and terrestrial magnetism is still being actively pursued; a striking result recently announced by E. W. Maunder from a discussion of long series of magnetic records at Greenwich and elsewhere, being that there is a marked tendency for a disturbance synchronous with the appearance of a sun-spot, to be repeated when the sun-spot has been carried round through a whole rotation of the sun. Instances can be found when a disturbance has shown in this way for several successive rotations. There is a strong inference that the solar disturbance manifested by the spot is the direct cause of the magnetic disturbance, though the *à priori* improbability of any solar emanation being directed in a stream to such a distance as that of the earth is confidently asserted.

Still more recently, Mrs Maunder, analysing the records of several years' sun-spots, has found that consistently greater spotted area is shown in the half-disc coming into view with the rotation than in the half-disc disappearing, a suggested inference being that the spots on the side towards the earth are affected by that circumstance. The idea of a planetary control for the spots is not new, though Jupiter, on account of its bulk and similar period (between 10 and 11 years) is generally suggested in this connection, but it is very evident that more analysis will be necessary before definite conclusions can be drawn.

The periodicity of solar phenomena is not confined to the spots, but is equally conspicuous in the faculæ and prominences, and one obvious step in the tentative solution of the problem is to regard all these manifestations as due to a regular variation of the solar radiation. Much work of a very exhaustive character has been done in the endeavour to determine what is called the "radiation constant," or in other words, to find what the direct heating effect of the sun would be on unit area of the earth without the interposition of the atmosphere. Given a reliable value for this, continuous observation of the variation, if any, would ultimately disclose any existing periodicity; and many are the forms of special instruments devised for this special purpose, ranging from the simple thermometer with "black bulb in vacuo," and the actinometers of Balfour Stewart, Violle and others, to the differential pyrheliometer of Ångström, all of which indicate with more or less accuracy a certain amount of variation; the most striking advance, however, in the determination of the "constant" is mainly due to the spectro-bolometer of Professor Langley, whose death in February 1906 was a great loss to science and particularly to the Smithsonian Institution of Washington. He had the satisfaction before his death of seeing the inauguration in a specially favourable position of a well-equipped solar observatory at Mount Wilson, California, where work on the lines he had begun can be carried on under the best conditions. Other solar observatories are also at work, a recent example being at Kodaikànal in Southern India, while those of Meudon (Paris) and South Kensington have for a longer time been working principally at solar research. Before leaving the subject of radiation it may be well to indicate some of the points that render the problem one of peculiar difficulty.

On the assumption that heat as such comes from the sun to the earth, which is not quite universally admitted as an axiom, it is propagated through the solar atmosphere, through what is often called the "ether" or interplanetary space, and through the earth's atmosphere. It may not be propagated similarly in all three media, and, without assuming any variation, such as is exceedingly probable, in the sun's atmosphere, the distance travelled through the "ether" has, at any rate, an annual variation; and the earth's atmosphere, or, at least, the lower part of it, is subject to such great variations in height and density, in circulation, in humidity and composition, each varying element having its own peculiar method of dealing with the radiation, that an enormous number of observations seems to be demanded in order to reduce the number of unknown quantities concerned. Moreover, an increase in the heat radiated from the sun would not necessarily involve an increase in the heat received at the earth's surface, for one of the first effects might be increased evaporation, and as this generally means increased cloud, a larger proportion of the sun's heat might be prevented from reaching the earth. The recently established International Union for Solar Research will run no risk of failing for lack of work.

It has lately been doubted by some investigators whether the sun's actual diameter is not subject to some law of variation. It does, of course, as viewed from the earth, appear larger when nearer the earth, but apart from this annual variation it is suspected that the discordances in actual measures, made by different observers, as for instance in the daily meridian observations at Greenwich, are not entirely due to systematic error depending on the altitude of the sun, or to accidental error due to the personal idiosyncrasies of the observer or the state of the air. It is only reasonable to suppose that changes in the radiation of the sun would affect the distance from the sun's true surface of the bright masses that we actually see, so that there is no inherent improbability in a genuine variation of the sun's diameter, besides that very slow shrinkage which is said to be taking place at a rate that would take thousands of years to prove. It has been recently stated by Dr C. Lane Poor that the polar diameter exceeds the equatorial at times of minimum activity, and falls short of it at maxima, but the varying velocity this would cause is difficult to trace.

Mention of the varying distance of the sun reminds us of the need for determining that distance. We have seen how far this problem of the solar parallax had progressed up to the middle of the nineteenth century, and how Hansen and Le Verrier gave reason to suppose that the definitely accepted value derived from Encke's rediscussion of the transit of Venus observations of 1761 and 1769 was incorrect. Not many years afterwards, their plea for a diminution of the sun's mean distance from 95,000,000 miles to about 91,000,000 miles, was independently supported by the ingenious determinations by Fizeau and Foucault of the velocity of light by means of revolving mirrors, repeated in 1874 by Cornu and in 1879 by Michelson of the U.S. Navy. The new determination was formally adopted in 1864, and Encke's value discarded. Astronomers were, however, loth to reject the observations used by Encke, and first Powalky (1864) and then Stone (1868) rediscussed those eighteenth century transits with improved values for the longitudes of the stations and using greater discrimination in "weighting" the different observations, and thus had the satisfaction of deducing results nearly in accordance with the newly-adopted value. Naturally the fast approaching transits of 1874 and 1882 were eagerly awaited as offering a splendid opportunity of clinching matters and obtaining a result free from suspicion of inaccuracy. As early as 1857 programmes and schemes of observation began to be drawn up, and the relative expediency of Halley's and Delisle's methods once more argued. Also, since photography had now come into general use, it was hoped that permanent results, free from physiological or psychological errors, would accrue in such numbers that extreme accuracy could hardly fail to emerge in the results. After years of discussion and preparation, including special training of observers, after the outlay of something like a quarter of a million sterling on some eighty stations, ranging from Japan to Mauritius and Kerguelen Island, occupied by parties representing nearly every nationality with any claims to scientific progress, and after generally favourable weather, so that a great mass of material was accumulated for discussion, it had to be confessed that discordances were nearly as great as before. Precautions had been taken to minify the effect of the "black drop" which had been noted on previous occasions, but the great phenomenon that marked the 1874 transit was the disturbing effect of the atmosphere of Venus. The improved optical instruments only served to emphasise this, and observers close together disagreed by sometimes twenty or thirty seconds in their estimate of the time of apparently identical appearances. As for the numerous photographs, except some taken by the Americans with long focus lenses, to avoid the necessity of magnifying the image, all were practically useless, as no measures of precision could be made of them owing to the indistinctness of the images. The net result seemed to be to increase slightly the probable error of the accepted value instead of largely reducing it as had been confidently expected. In 1877 a favourable opposition of Mars took place, and Dr Gill (since knighted, and who only in 1906 resigned the important post of His Majesty's Astronomer at the Cape) took a fine heliometer to the Island of Ascension, and observed the position of Mars in reference to neighbouring stars, morning and evening, in order that the rotation of the earth should provide him with the necessary base-line, a device due to Airy, but not actually employed before. His result, giving a distance of 93,000,000 miles, was received with great confidence and honoured by the bestowal of the Gold Medal of the Royal Astronomical Society. The size of Mars is, however, a disturbing factor, and favourable oppositions are comparatively rare; and better measures were considered probable if one of the brighter minor planets with a distance not much greater than that of Mars were observed instead. Some few determinations by the "latitude" method, using different minor planets, and one by Airy's "diurnal" method, had already been made.

The transit of Venus in 1882 was not regarded with the same enthusiasm as the previous one. Some countries practically ignored it, on the ground that the "minor planet" method was better, cheaper, and of more frequent application; others, though intending to observe it, did not adhere to an international plan. The ultimate result of all the different methods, the photograph, the heliometer, and eye-observations of contacts was once more an array of discordant values.

Gill in 1888 and 1889 applied a modified plan of campaign with great precautions to Iris, Victoria, and Sappho, the idea being to double the observed displacement by simultaneous observations made at opposite sides of the earth when the planet was just between the stations. Several heliometers were used in co-operation, but the result is generally known as that of Gill and Elkin (of Yale College). Several years passed before this result was published; and meanwhile, in 1890, yet another redetermination was made, this time by Professor Newcomb, from the transits of 1761 and 1769, and the result agreed closely with the best obtained by other methods. Newcomb had some years previously redetermined the velocity of light with a Foucault apparatus on a large scale, his mirrors being more than two miles apart instead of in one room; and his result, being far more accurate than any previous one, after having been employed to deduce the solar parallax from the constant of aberration, was employed by Gill in connection with his Iris, Victoria, and Sappho results, published in 1897, to deduce the constant of aberration from the solar parallax. The very next year, 1898, witnessed the discovery of a remarkable minor planet, which known first as 1898 D Q, and subsequently as (433) Eros, was destined to reopen the question of the solar parallax once more. Once in thirty years this planet comes even nearer to the earth than Mars, so much of its orbit lying within that of Mars that the

French do not include it in their list of "planets between Mars and Jupiter." The advantages a favourable opposition of Eros would provide for the redetermination of the solar parallax were so great that a programme was drawn up by an International Conference at Paris, by which the next comparatively favourable opportunity, in 1900, should be utilised to the full by many co-operating observations, relying mainly on series of photographs showing the apparent motion of Eros among the stars, which could be measured and reduced at leisure and discussed all together. Many photographs have been taken, and after much preliminary discussion some of them have been measured and the results compared; but it cannot be said that the work is complete, though doubtless when the most favourable opposition arrives it will be found that this pioneer work now in hand will save a great deal of time then. Eros is not the only hope of the solar parallax problem, for at least two other lines of advance remain. Every advance in the lunar theory, and in the accuracy of the coefficients of the various inequalities, must bring us nearer to the exact values of the constants involved, and one of these is the solar parallax. Work is being done in that direction, and Cowell of Greenwich Observatory has in some preliminary papers given results which point strongly to the rehabilitation of this method of determination. One other promising side from which the problem is being attacked is the spectroscopic side. We shall in the next chapter be dealing more directly with spectroscopy, so it must suffice here to note briefly that one of the fruits of the application of Doppler's principle is that the motion of distant stars in the direction of the observer, or as it is called the "line of sight," or "radial" velocity, can be measured; and it has been recently pointed out that by using careful discrimination in the choice of stars, and of the time of night and time of year for observing their spectra, it is possible to obtain equations giving not only the radial velocity of the star chosen, but also the velocity of the solar system in space and of the earth in its orbit, from which last the distance of the earth from the sun, or, in other words, the solar parallax, can be deduced. This method has already been tried to some extent by Professor Küstner, of Bonn, and is still in the field, at the Cape Observatory, among others; but its success in comparison with other methods depends entirely upon the degree of accuracy obtainable in the measures of radial velocity, and it is doubtful if even a large number of independent observations will give a result with a very small probable error. To attain striking success, it must be possible to reduce the probable error of the determination to distinctly less than one part in a thousand, at which it is supposed to stand at present.

Another elementary idea in connection with the sun is its rotation, the discovery of which was a necessary consequence of that of sun-spots. Galileo himself vaguely indicated the period as "about a lunar month," and Scheiner gave twenty-seven days, both these values being meant for the apparent period, the true being some thirty odd hours shorter in consequence of the earth's orbital revolution taking place in the same direction as the sun's rotation. Numerous values of the true period were obtained from time to time ranging from 25 to 25½ days, no one for two centuries, except Scheiner, letting fall any hint as to the period being variable. Scheiner noticed that different spots gave different periods, the longest he obtained being from a spot in a higher latitude. Ultimately, however, it was pointed out by C. H. F. Peters in 1855 that a careful series of observations made at Naples ten years before showed unmistakably that spots were not fixed, and consequently could not be expected to agree in giving an exact value of the rotation period. At this time Carrington, who had been building himself an observatory at Redhill with the intention of providing from his own observations a catalogue of circumpolar stars, took up also the daily observation and measurement of position of sun-spots (which had been recommended by Sir John Herschel), so that his days as well as his nights might find him astronomical employment. He made the important discovery, hinted at more than two centuries before by Scheiner, that the period of the sun's surface rotation increases from rather less than 25 days at the solar equator to about 27½ days for spots at the highest latitude at which they occurred (about 50°). He represented the variable rotation by an empirical formula without any attempt to explain the cause. He also determined with great accuracy the direction of the sun's axis of rotation, which points about half-way between the present pole star and the brightest that can ever have held that position, Vega or α Lyræ. Another result of his labours was the notable connection between the mean latitude of sun-spots and the epoch in relation to the sun-spot period, the "spot zones" on either side of the solar equator gradually contracting towards it as the minimum approached, the zone of maximum being at about 16° latitude, and each successive series dying away at about latitude 6°, while the new one was often already beginning near latitude 35°. Professor Wolf of Zurich, whose historical knowledge of sun-spots was unrivalled, soon found confirmation in previous observations, and Spörer and Secchi proved the truth of the law at the next minimum in 1867. Spörer, whose name is generally given to this "law of zones," was working at Anclam in Pomerania, and independently discovered the variation of rotation with latitude about two years later than Carrington. He unearthed evidence of the truth of the "law of zones" for many previous minima, the earliest being that of 1619, but found that for about seventy years, 1645-1716, there were very few sun-spots at all, and consequently no apparent law. On the foundation of the new Astro-physical Observatory at Potsdam in 1874 he was appointed to the staff there, and worked at sun-spots until his death in 1895, twenty years after that of Carrington. But a powerful instrument was already being perfected while Carrington was at work, which was destined to throw more and more light on solar mysteries.

# CHAPTER XVIII

## SOLAR SPECTROSCOPY

WHEN Newton analysed the sun's light by passing it through a glass prism, and after isolating a beam of one colour found that a second prism analysed no further, he was taking an early step in the vast field of Spectrum Analysis. In a small book like this it would be quite impossible to do justice to a domain which brings physics and chemistry into intimate fellowship with astronomy, and under the names of astrophysics and solar and stellar chemistry shows vast fields for investigation in directions beyond the wildest dreams of the old mathematical and dynamical astronomers. It would, however, be equally impossible to omit spectroscopy altogether. For our present purpose it must suffice to note that spectroscopy in the modern sense dates back only to the time of Waterloo, when Fraunhofer, the great Munich optician, produced a map of the solar spectrum containing hundreds of dark lines, measuring and naming the more conspicuous ones with letters of the alphabet still in use for the purpose; for instance, D for a prominent line in the yellow part of the spectrum. This same D line he found in several of the brightest star-spectra.

Laboratory spectroscopists by examining light from different sources, incandescent metals, burning gases, or sparks from metallic electrodes, were enabled to lay down a few elementary laws, as, for instance, that an incandescent solid or liquid body yields a continuous spectrum without any dark lines, and that certain groups of lines are identified with certain gaseous substances, the famous D line, for example, belonging to sodium. In 1832 Brewster discovered that certain lines became conspicuous as the sun was observed at low altitudes, and these he rightly attributed to the earth's atmosphere, suggesting moreover that the sun's atmosphere might account for the vast majority of the lines which were unaffected by the altitude of the sun. This shrewd guess was, however, apparently contradicted by Professor Forbes, who, at the annular eclipse of 1836, found the light from the rim of the sun to give exactly the same spectrum as that from the centre, whereas he argued that if the sun's atmosphere has any absorbing effect it would be greater for rays from the limb. Why this is not the case is still unknown.

In 1859 Kirchhoff found that sodium vapour which ordinarily gave a bright D line, only intensified the dark D line in sunlight sent through it; and, moreover, produced a dark D line in the otherwise continuous spectrum of a Drummond light similarly sent through it. It was, therefore, obvious that sodium vapour exists in the sun's atmosphere, and its absorption of rays of its own characteristic wave-lengths or refrangibility deprives the solar spectrum of those rays, leaving relatively dark lines. The great question was at once settled, and the list of elements recognisable in the sun's envelope soon came to include not only sodium, but also iron, calcium, and several others in greater or less quantity. Kirchhoff may then be regarded as the father of astronomical spectroscopy. From the chemist's point of view his name and fame are associated with his co-worker Bunsen, but Bunsen's work was confined to the laboratory. Kirchhoff's eight-foot map of the solar spectrum, published by the Berlin Academy in 1861 and 1862, was at once accepted as the correct interpretation, and solar chemistry was henceforth a recognised and important branch of science. Kirchhoff's law that radiation (or emission) and absorption spectra are the inverse of each other at the same temperature had been almost, but not quite, anticipated by more than one scientist.

Comparison of the spectra of terrestrial elements with those of the sun, stars, and other celestial objects gives in this way information as to their chemical constitution, but it effects far more than a simple qualitative analysis, for the behaviour of the lines is not invariably the same; and much may be divined of the temperature and pressure of some of the gaseous constituents, from the changes in width and intensity of characteristic lines. Moreover, in yet another direction has new and valuable work been rendered possible.

In 1842 Doppler published his "principle" referred to in the last chapter. Sound and light are both propagated in waves, and the frequency of the vibrations determines in the one case the pitch, and in the other the colour, both being impartially described by the word "tone." But if the observer and the source are in relative motion that motion will have the effect of altering the frequency. The stock example in the case of sound is the whistle of a passing locomotive, which to the driver and bystander gives the same sound only at the moment of passing, being of higher pitch to the bystander before passing, and of lower after. The difference, depending on the velocity of the train, is easily recognisable in the case of sound, which travels at less than twenty times the speed of an express train, and Doppler stated that the same principle would be manifested in the behaviour of light-waves, with a velocity nearly a million times greater. This has been found practically true in the case of the Fraunhofer lines, though the absence of a simple and invariable reference mark tends to conceal the effect in the case of continuous light.

The first results of the application of this principle to the determination of radial velocities of stars were communicated to the Royal Society by Sir William Huggins on St George's Day, 1868. Three years later, at the suggestion of Zöllner, who had in the meantime devised his reversion spectroscope to double the effect to be measured, Professor Vogel of Bothkamp succeeded in applying the method to the sun's rotation. The sun's poles may be regarded as relatively fixed for this purpose; but the rotation velocity, though small, being in opposite directions relatively to the earth at the eastern and western limbs, is sufficient to give a very slight want of agreement between spectra from the two limbs viewed together, the only lines which fit exactly being those due to the absorption by the earth's atmosphere (the telluric lines), which are, of course, unaffected by the sun's rotation, and which can be mapped out by this method.

The effect of work on these lines by Professor Young of Princeton, Langley of Allegheny (afterwards of the Smithsonian), Cornu in France, Thollon at Nice and others, was to establish the validity of the method as one of very great refinement. It was but a step to apply it to the anomalous solar rotation periods of Carrington and Spörer. Dunér, a Swedish astronomer, pushed the method far beyond the spot zones, and detected a rotation period at 15° from the sun's pole of 38½ days as against 25½ days at the equator. His results gave rather longer periods than those deduced from observations of spots; in fact it appears that the periods are distinct, and, moreover, the determination from faculæ gives a still quicker period than that from spots. This should cause no surprise, for the axial rotation effect, being more marked where the distance from the axis is greater, seems to demand a slackening at higher latitudes and also at "lower" levels, and it is by no means improbable that the existence of well-marked frequency zones for spots is only another effect of the same cause: the gradual variation with latitude of the depth and velocity of different strata. According to this, solar disturbances might be regarded as indications of weakness or instability where the velocity and the rate of change are both comparatively large. Not to labour the point, we have an equatorial zone with no rate of change and practically no spots; polar zones with lower velocity and no spots; and intermediate regions with velocities nearly as great as the equatorial one and yet changing with latitude, and here we do find spots. The suggestion falls far short of accounting for all the phenomena of the spot-cycle, but no solar theory has yet been found beyond criticism.

But the sun's rotation is only one of the problems to which the new method of research is applicable. Reference will be made in the next chapter to the discovery made in 1868 by Janssen and Lockyer, and theoretically also by Huggins, that solar prominences could be studied without an eclipse, and for many years Lockyer studied the line-of-sight motion of prominences; Young in America, Fenyi in Hungary, and Trouvelot at Meudon following in a similar path. They have actually seen outbursts travelling at hundreds of miles per second, and vast clouds, hundreds of times larger than the earth, rent to fragments in a few minutes, and have proved practical coincidences between some of the great solar storms and terrestrial magnetic storms and auroral displays. And after observation, theory. The first result of much of Lockyer's work was the suggestion that what are known as chemical elements, inasmuch as terrestrial chemistry has failed to analyse them any further, are not really so, but are simply very refractory compounds, which can be and are dissociated in the almost inconceivable temperatures of our own and other suns. Whether the ultimate tendency of this suggestion would be to reduce all substances to variants of one primal element, or by splitting up the known "elements," some seventy in number, largely to increase the number of ultimate radicles, is, of course, problematical, though from the complexity of some spectra the latter seems the more probable. But in so far as the argument rests on the behaviour of certain characteristic lines under the stress of enormous forces and temperatures, or even on the presence of so-called "basic" lines in otherwise different "element" spectra (pointing to a common constituent), the case is far from being even approximately proved. The tendency of more modern research is to discredit the idea of dissociation by heat in favour of some radiative or electro-magnetic effect called "ionisation."

Maps of the solar spectrum have advanced steadily since Kirchhoff's time, and not only those of the visible spectrum but of the invisible parts, where the vibrations are either too quick or too slow to impress the human retina as light. The very quick ultra-violet portion was mapped first by Henry Draper in 1873; the slow infra-red region, for some time gradually extended by the researches of Abney and Festing and of K. Ångström, has yielded magnificent discoveries to Professor Langley's extremely sensitive differential bolometer, and appears to be of vast extent. Scheiner suggests that those slower vibrations merge gradually (rather than ending) into Hertzian waves.

The standard map of the spectrum is that of Professor H. A. Rowland, who, having in successive editions so largely increased the number of measured lines in the spectrum that identification of elements failed for lack of sufficient comparison spectra, spent the rest of his life in photographing the spectra of every known element he could obtain and comparing them with the solar lines, some 16,000 in number, in his great map. He died in 1901, with the work unfinished; but Hasselberg, Kayser, and others have worked in the same field, and there is no lack of investigators to carry on so obviously important a branch of research. Atmospheric lines have been studied almost to perfection. By 1878, very greatly owing to the work of Sir Norman Lockyer, the number of elements provisionally recognised in the sun reached 33, the first, hydrogen, having been discovered in 1862 by A. J. Ångström, and 13 more having been added to the list before Lockyer began his research. If we consider hydrogen as a metal, the whole 33 were metals, and the first non-metallic element proved to exist in the sun was carbon, which, after partial evidence had been adduced by several successive investigators, was rendered certain by Rowland, who also found another non-metal, silicon; and, though discarding some of the "provisional" identifications, also slightly increased the number of metals proved to exist in the sun.

It is now considered practically certain that oxygen exists in the sun, though its alleged discovery by Henry Draper in 1877 has long been disproved. Janssen proved that nearly all the oxygen lines in the spectrum were undoubtedly of terrestrial atmospheric origin, but a few oxygen lines low down in the red have been adjudged to be on a different footing.

A recent development of this spectral analysis of the sun has been the study of different layers of the sun's atmosphere in monochromatic light, that is to say, by using a second slit, so that only a narrow portion of the spectrum is able to reach a photographic plate, and by allowing the sun's image to travel across the first slit while the plate travels behind the second slit, thus building up strip by strip an image of the sun in, for instance, the K light of calcium. Professor Hale, of Chicago, has thus obtained very interesting pictures, showing the distribution of calcium "flocculi," and the method admits of great extension. It is already practised with success at South Kensington, and it is hoped that the new solar observatory at Mount Wilson, California, will in this, as in other ways, largely increase our knowledge of this fascinating branch of analysis.

Within the limits of this book it is not practicable to enter at any length into the development of the various forms of the spectroscope itself. What is requisite in all forms is something which will separate light of different wave lengths, as widely as possible, without too great loss of light. A single prism does not give very much separation, and a prism train absorbs more and more light as the number of prisms is increased. Rowland's diffraction grating substitutes for the prism a reflecting surface ruled with a very large number of fine lines very close together, so that by the principle of interference duplicate rays are eliminated and a "diffraction" spectrum produced. Rowland's practical limit is 43,000 lines to the inch, ruled on a concave surface, so that the spectrum is brought to a focus without any absorption by an object glass, though there is a large amount lost by reflection and scattering. The idea of a grating is not Rowland's, for it was in use in Fraunhofer's time, the light being transmitted through an actual wire grating; but Rutherfurd and Rowland introduced, and the latter perfected, a system of machine-ruling, for which, to avoid periodic error, a "perfect" screw was requisite. They also discontinued the use of transmission gratings, thus slightly diminishing the loss of light. It was long, however, before gratings could be employed for anything much fainter than the sun, though in this direction, as we shall see later, the increased power of the modern telescope comes in with great effect.

# CHAPTER XIX

## SOLAR ECLIPSES: SPECTROSCOPY

IT is perhaps more usual to postpone the consideration of spectroscopy until after that of the increase of knowledge obtained from solar eclipses. It is so difficult to study the two separately, however, that perhaps the order is not very material. In Chapter XIII. we glanced at some eclipse phenomena noted up to the time of the eclipse of 1842. A few years later, the first daguerreotypes of the sun were taken, one at the eclipse of 1851, but solar photography with collodion plates practically began with the work of Warren de la Rue, whose observatory at Cranford was built for the purpose of celestial photography in 1857, only six years after the invention of the collodion plate, and in 1858 Kew Observatory commenced a long series of solar photographs. At the Spanish eclipse of 1860 De la Rue and Father Secchi at different stations succeeded in obtaining photographs, showing beyond doubt what had been only regarded as a probability, that the rosy prominences belonged to the sun and not the moon, inasmuch as the motion of the latter was shown across the background of prominence light, and also showing the reality and comparative permanency of those flames, the eclipse taking place seven minutes later at one station than at the other, and yet showing recognisable identities.

A still greater advance was directly due to the eclipse of 1868 in India and the Malay Peninsula, for which the spectroscope was added to the equipment of the eclipse expeditions. The bright lines known to indicate incandescent vapour were seen by several observers, and though not all at once identified, the presence of hydrogen was clear, and also a bright line noted in the "orange," which was at first assumed to be the D line of sodium, already known to be double ($D_1$, and $D_2$). Janssen, moreover, who was one of the successful observers, was so struck with the brightness of the lines he saw that he at once announced that he would see them after the eclipse was over. This prediction he fulfilled next day, carrying out the idea suggested to his fertile brain during the eclipse. This idea sounds very simple, like the egg feat of Columbus, when somebody else has pointed it out. It is not actually the brightness of the sun's disc that ordinarily prevents the prominences on the limb from being seen, but it is the brightness of the rest of the field, that is to say, the diffused sunlight in the sky. It follows then that when this is withdrawn by the interposition of the moon, we can see the prominences easily; but it also follows that if we can diminish the relative brightness of the sky, we should get a modified form of the same effect. The use of a spectroscope, while diminishing the diffused light by spreading a given quantity over a large

PROMINENCE ENLARGED. 1900

CHROMOSPHERE. 1905

space, only refracts the prominence bright lines, so that in comparison with the background they become brighter with increased dispersion. This notable discovery had however been anticipated. Lockyer had for two years been awaiting the completion of an instrument ordered for the express purpose of viewing, under high dispersion, the bright-line spectrum he *expected* the prominences to furnish, and receiving it after news of the eclipse had shown what lines should be there, he very speedily found them. A delay of a month by Janssen before sending news of his discovery to the Paris Academy of Science allowed Lockyer's to be received a few minutes earlier. The independence of the two identical discoveries was at once admitted, and both names were equally honoured, and the Academy's gold medal for the year awarded jointly.

Failing these two scientists, yet a third was on the track earlier still, and there is little doubt that in time Sir William Huggins would have been able to announce that he had discovered what he had sought some months before Lockyer obtained his instrument or Janssen viewed his eclipse, and that had it been known beforehand what bright lines would be found, he would certainly have obtained priority. But as we have seen, it was not until after the eclipse had come and gone that anyone knew exactly where to look.

It was no longer necessary to study prominences in the few precious moments of an eclipse, so more time could henceforth be spared on such occasions for the corona, whose spectrum was hardly seen in

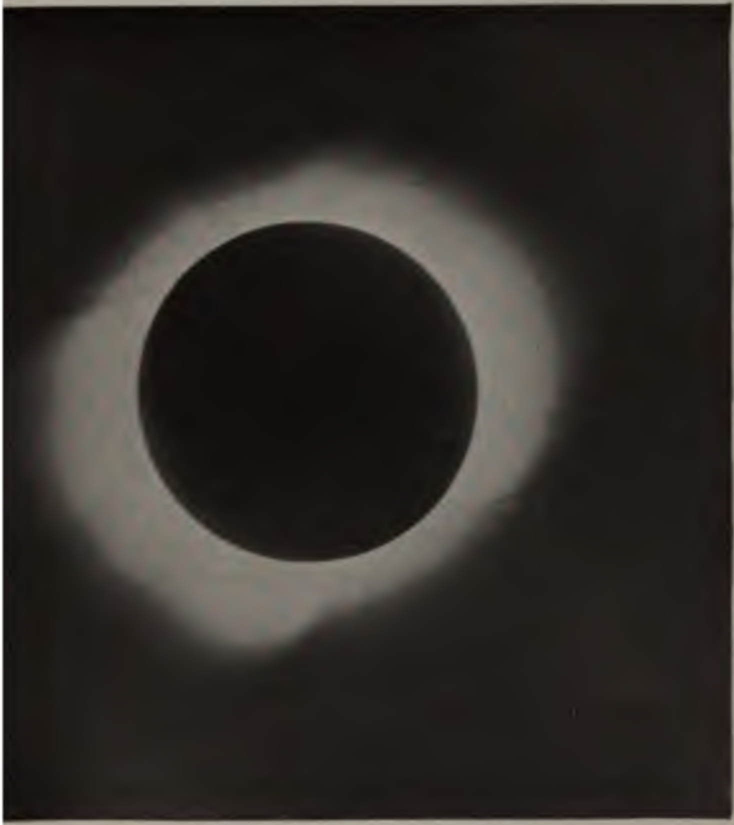
1868, though it was inferred from its appearing polarised in planes through the sun's centre that its light was in part reflected sunshine. In the following year, at the North American eclipse, a continuous spectrum was seen with a single green ray, at first identified with an iron line, 1474, in Kirchhoff's map; but since 1898 recognised as a distinct ray due to a substance unknown on earth, but called coronium, and considered in virtue of its persistence at 300,000 miles from the sun's surface, to be much lighter than any known terrestrial substance.

The next eclipse, of December 1870, is memorable in many ways. Janssen left Paris in a balloon, in order to escape the besieging Prussians, but from his station in Algiers was entirely prevented by cloud from seeing anything of the eclipse. Lockyer was shipwrecked on the way out to Sicily, and only saw the eclipsed sun for a second and a half. But Professor Young, one of those who had measured the green ray in the previous year, was more successful. At the instant of totality, the spectrum, which had been fading gradually away as the light diminished, was suddenly reversed, every dark Fraunhofer line being replaced by a bright one. This discovery of the "reversing layer" was due to the employment of a slit in a direction tangential to the disappearing limb of the sun. Its appearance was theoretically expected, as it was assumed that some layer, cooler than the actual sun, was responsible for the absorption of those same lines, and would, if the sun's light were withdrawn, give those very lines bright. The spectrum of the "flash" has been often seen since. The "layer" is so thin that three seconds is apparently an outside limit to its visibility. A "snap-shot" of it, however (and without some such permanent record its character could hardly be considered absolutely proved), was one of the fruits of the unfortunate eclipse of 1896, though the success then achieved at Novaya Zemlya by Shackleton has been often repeated since.

In 1871 December, an eclipse, visible in India and Australia, gave Janssen another opportunity, of which he, in the pure atmosphere of a hill station in Southern India, made such good use that he detected dark Fraunhofer lines, including the D line of sodium, in the spectrum of the corona, using an instrument of short focus and large aperture in order to give a very bright image.

The next advance was an instrumental one, already attempted in 1871, by the substitution of a prism outside the object glass for a slit at the focus of the spectroscope. The outcome of this was what is now known as the prismatic camera, employed first in 1875, and at nearly every observed eclipse since that time.

There is an underlying truth in the myth of Hercules and the Hydra as symbolical of the struggles of scientific investigation towards the elucidation of problems. Each advance in the power of the scientific weapon discloses new fields of inquiry at least as fast as it reaps harvests from the older ones. The detailed analysis of the chromosphere and corona at once suggested the general question as to variability or permanence in the solar envelopes. The obvious test was a comparison of the features of an eclipse at times of greatest and least solar energy, as evidenced by spot-activity. The general form of the corona near a maximum sun-spot epoch had been noted in several eclipses; the extension of the corona being then relatively great in the "spot zones," making the external boundary a rough square, or rather oblong, since the mean latitude of spot zones is far below 45°. In 1878, at a minimum sun-spot epoch, the appearance was quite different and extensions were seen in the direction of the ecliptic. These were variously assumed to be swarms of meteors, or else the zodiacal light (if indeed these are really different explanations); while another typical feature of the corona was a brush-like structure at each pole, of a distinctly magnetic appearance, inasmuch as in each case it seemed to radiate from the pole and not from the sun's centre. This eclipse was viewed under exceptional conditions from Pike's Peak, Langley even occupying the summit more than 2½ miles above sea-level, so that the purity of the atmosphere was far greater than had usually been the case at eclipse stations. The general type of corona agreed with what had been recorded on at least one previous occasion of minimum activity. The spectrum also differed from that of the "maximum" corona. The green coronium ray was far less conspicuous, and the polarisation diminished outwards from the limit instead of first increasing to a maximum. This eclipse is notable for the alleged discovery of intra-Mercurial planets by Swift and Watson.



MINIMUM CORONA, 1900

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In 1882, a year of great sun-spot activity, came another opportunity of contrasting a "maximum" corona with the "minimum" features of 1878. In the pure air of Upper Egypt the brushes and long streamers were not seen, but the structure once more resembled the corona of 1871. The polarisation at the limb was less, indicating a smaller proportion of reflected light. This eclipse is unique in that a photograph of the corona shows a comet close to the sun, which is supposed never to have been visible before or since, but there is no certainty on the subject. The H and K lines of calcium appeared so strong in the coronal spectrum of 1882 that Huggins hoped by cutting off the rest of the light to photograph the corona without an eclipse. By using silver chloride plates which did not react to the bright part of the spectrum he obtained promising results, until the great eruption in the Straits of Sunda in 1883 filled the upper atmosphere with dust, causing magnificent sunsets, but sadly interfering with the transparency of the air, on which delicate solar observations so largely depend. From time to time various devices have been tried since 1883; for instance, at Pike's Peak in 1893 by Professor Hale, and on Mt. Etna in the following year by Professor Ricco; but the verdict was failure, and it is only quite recently that a confident claim of success has been made by A. Hansky on Mt. Blanc.

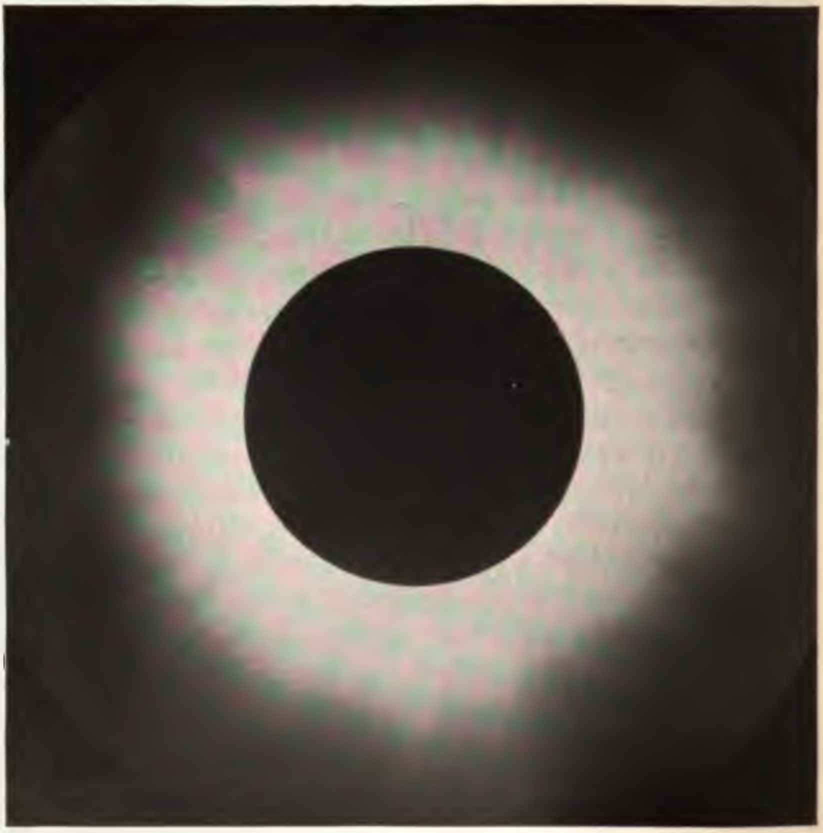
Much has been written to prove that the outer corona at any rate is not real, but is a diffraction effect due to the earth's atmosphere and the rifts in the moon's limb. Lockyer, for instance, many years ago argued from spectroscopic evidence as to the extreme tenuity of the gases in the atmosphere, that there can be no pressure there sufficient to support an extended corona. But since the corona as viewed from a mountain top shows far more extension than when viewed from a lower station, it is evident that atmospheric glare is not the cause of the phenomenon.

The New Zealand eclipse of 1885 was signalised by the observation of two "white" prominences. It is only the rosy prominences, or it may be only the rosy interior portions, that can be seen without an eclipse, and Tacchini in the following year emphasised this still more strongly. The corona that year was of an intermediate or transition type, and in the following year the little success granted by the weather, which was bad over most of the eclipse-track, showed a still further transition towards the minimum type, which was reproduced in both eclipses of 1889, the main difference between them being an apparent east to west reversal of the widest extension. The later one cost the life of one indefatigable solar observer, Father Perry of the Jesuit College at Stonyhurst, who died of malaria contracted in the damp heat of the Salut Islands off French Guiana. The Royal Astronomical Society, who sent him out, also sent to the west coast of Africa, it being considered of great importance not only to increase the chances of success by occupying more stations, but by choosing those differing much in longitude, to test what changes, if any, could be detected during the interval of 2½ hours between the occurrence of the phenomenon at the two stations. Father Perry's death greatly diminished the value of his photographs, as he was unable to develop them at once, and they did not keep well in that moist climate. Taylor, at the other side of the Atlantic, saw nothing of the eclipse owing to cloud. A similar opportunity occurring in 1893 was favoured with fine weather from the Andes to the African coast. Schæberle of the Lick Observatory obtained a comet-medal on this occasion for a curious paraboloidal form mixed up with many coronal streamers on one of his photographs, but many doubt whether the appearance was not simply a solar appendage. Fowler, at Fundium on the African coast, made good use of the prismatic camera, finding not only the known green ray, but seven others, none of them identified, possibly all belonging to the substance which has been named coronium; but of lines belonging to a spectrum like that of the chromosphere, or prominence layer, he found none, suggesting that no terrestrial element such as hydrogen or calcium occurs in gaseous form in the corona. The rotation of the corona was sought in vain by Deslandres, since the violet rays he wished to employ for this purpose from opposite sides of the sun were absent.

We have already referred to the unfortunate eclipse of 1896, when most of the observers in Norway and Japan were entirely disappointed; but by the generous offer of Sir George Baden-Powell, an English expedition was enabled to meet with success at Novaya Zemlya, an island also occupied by a Russian party, including Hansky, who drew the inference from his observations that in every case an eruption in the chromosphere by its expelling force was responsible for each streamer from the corona.

The Indian eclipse of 1898 was favoured with very fine weather, and good photographic conditions, evidenced by Mrs Maunder's success in showing on a plate, taken with an aperture of only 1½ inches, the greatest length of streamer ever photographed, extending to 3° from the limb. Totality was short, only 100 seconds, and no results of definite scientific importance emerged, except in the promise of future success in two different directions. Professor Turner's suggestion of the advantage of using a cœlostat, a plane reflector driven by clockwork so adjusted as to provide an image of the sun in a constant horizontal direction while the telescope tube remained fixed in shelter, was largely adopted at this eclipse, and found to work admirably. By the use of the cœlostat, a telescope of large aperture can be taken to a distant station without the necessity for also providing its heavy mounting, that required for the cœlostat being much less cumbersome. Moreover, in this way an objective can be used without its heavy tube, a temporary wooden framework to exclude light being all that is necessary to connect the lens with the camera, which can be in a dark room.

The other direction in which promise was shown was the attempt made by Professor Turner himself to photograph the corona in polarised light. In the



MAXIMUM CORONA, 1905

eclipse of 1901, stations in Mauritius and Sumatra gave additional evidence as to changes in the corona in a short time, and rendered it fairly certain that the polarisation effect belongs only to the outer corona, while the inner corona is self-luminous.

The eclipse of 1905 August 30, whose track crossed Labrador and Egypt, promised another excellent opportunity for detecting changes in a few hours by comparison of results at the two ends of the long arc. But though the conditions in Egypt were excellent, nothing was seen by any official party in Labrador. Valuable results were obtained at those of the Spanish and African stations which were favoured with good weather conditions, while others were little more fortunate than Labrador. For instance at Guelma in Algeria, Newall was exceptionally favoured and exceptionally successful, having for the eclipse the only fine day in the week, but even there the attempt to determine rotation of the corona by comparing the green ray at east and west limbs failed by reason of the faintness of the ray, which did not show on the photographs. Elsewhere it was noted that the polariscope showed a maximum effect at about 5 or 6 minutes from the limb, confirming previous observations near a sun-spot maximum.

From the eclipse of January 1907 not much was hoped, though the track from north of the Caspian nearly to the Gulf of Okhotsk was uninterrupted by oceans. It passed at the most unfavourable time of year over the "roof of the world," hardly any part of its length being in a promising region. As it was, however, the only eclipse for some years to come of any promise whatever, Russian, French and German expeditions were sent, only to meet with disappointment. The eclipse was seen, but through falling snow, which effectually prevented any delicate scientific observation.

We have little space for solar spectroscopy from the more strictly chemical point of view. It has long been known that the D line discovered at the 1868 eclipse is not the double line of sodium, but slightly more refrangible. Under the name of $D_3$ it has been attributed to a substance to which Lockyer gave the name of helium,[1] but in 1895 it was found by Professor Ramsay in a terrestrial mineral, cleveite; it has twice the atomic weight of hydrogen, the lightest known element. Other hitherto unknown lines in the spectrum of the chromosphere were soon recognised as belonging to helium. Many metallic elements are occasionally identified in the spectrum of the sun's "upper atmosphere," in addition to the always present hydrogen, helium and calcium, and it is thought that this indicates a simple density stratification frequently disturbed by solar activity, which temporarily upsets the equilibrium and permits the lower strata to be for a time unmasked.

One of the modern developments in eclipse work is the greatly increased scientific interest shown by amateur societies, such as the British Astronomical Association, which has been represented at nearly every practicable eclipse since its first definite expedition to Norway in 1896, which was, as we have seen, so unfortunate. It has been said with some truth, and perhaps some bitterness, that the more care is taken to select a station where the average weather conditions are best, the greater the probability of total failure. Certainly the only members of the Norway party in 1896 who saw anything of the eclipse were a few who did not trouble to go to the selected spot, but stayed at the first point reached by the expedition from which the eclipse could by any possibility be seen. There are so many possible lines of investigation for which elaborate apparatus is not required, that the assistance of a large number of comparatively untrained observers is exceedingly welcome, relieving the better equipped scientific expeditions of many details that would take valuable time which they can ill spare out of a period that in the most favourable case could not possibly reach eight minutes, and is rarely half so long. It has become customary for Sir Norman Lockyer, when in charge of a British Government Eclipse Expedition, to have the assistance of most of the officers and crew of a war vessel, and their keenness of vision, their trained intelligence and habits of discipline, render them peculiarly efficient for the purpose.

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[1] To mark it as a solar substance not known on the earth.

Of the accompanying phenomena of a solar eclipse, for instance, the apparent darkness (measured either by a photometer, by noting the faintest stars that become visible, or by some empirical method such as ability to read small print), the behaviour of

plants and animals, the appearance and velocity of the "shadow-bands," and many other matters provide a programme of great interest, and necessitate, for completeness, a fairly large eclipse party, in addition to the spectroscopists and other photographers, the physicists who measure the effect of the interposed moon on radiation or terrestrial magnetism, and the meteorologists who note the variation of temperature and humidity. Longer and longer grows the programme; practically nothing can be cut out as no longer worthy of notice, though "shadow-bands," for instance, are generally regarded as a phenomenon of physical optics, and it is hinted that they, as well as the celebrated "green flash," can be produced under other conditions than in the one case, a total eclipse, and in the other, the setting or rising sun.

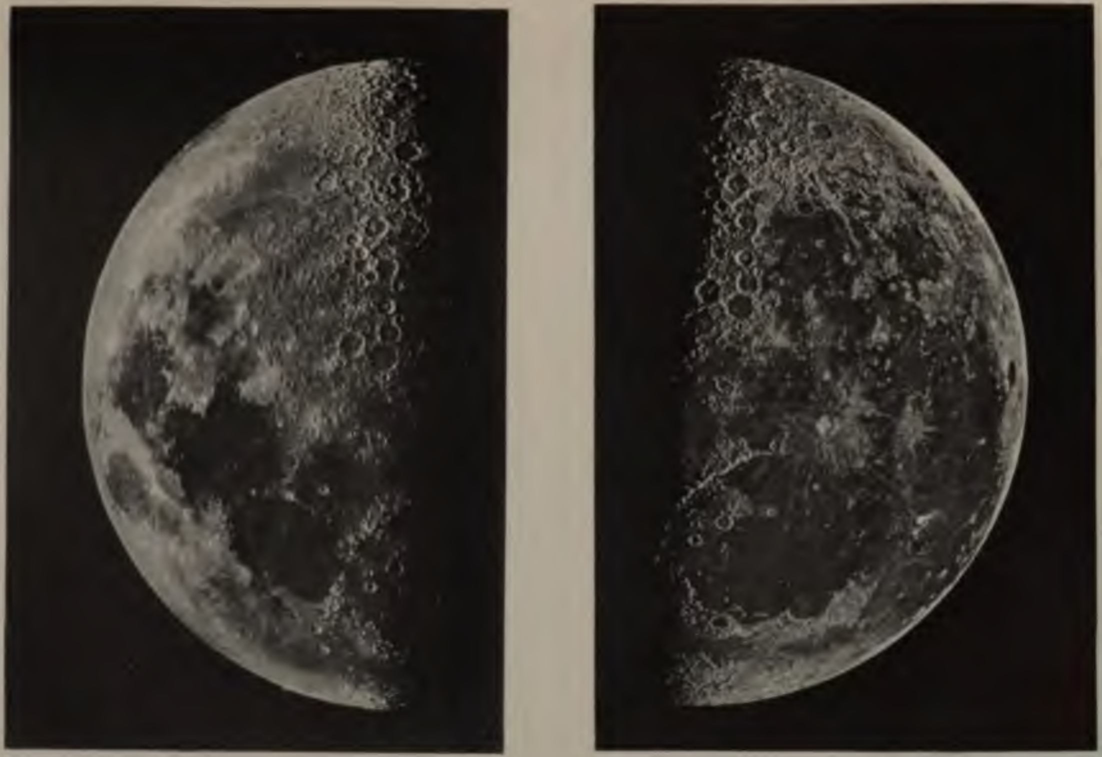
# CHAPTER XX

## THE MOON

THE moon, though nearer the earth than any other celestial body, and regarded as our own peculiar property, has from time to time suffered from a strange lack of interest among astronomers. Even Herschel did practically nothing in that direction, though very little else escaped him. We have noticed the progress of the theory, and now come to observation. As regards the position of the moon, it has for a very long time been considered the peculiar prerogative of the Royal Observatory at Greenwich to observe the moon on every possible day, the long series thus accumulated from 1750 or thereabouts providing a rich store of material for the completion or, at any rate, improvement of the lunar theory. As it is impossible in general to observe the moon on the meridian at Greenwich within three hours of noon, on account of its relative faintness against the bright sun-lit sky, it has been for more than half a century the practice to observe it off the meridian with an altazimuth for all possible days when the meridian instrument could not for the above reason be used, and also on sufficient days, often the whole month, when both instruments were available, for the sake of comparing the two and determining or eliminating systematic error, which was expected to be larger in the case of the altazimuth on account of its necessarily less stability. We have noted the new Universal Transit Circle, with which it has been possible still further to reduce this error, though its use in fixed azimuths somewhat lessens the number of observations.

One other recent modification remains. The observations of the limb of the moon, whether seen against a bright sky, a dark sky, or an intermediate twilight sky, are by necessity compared for fundamental purposes with observations of stars, which are of a distinctly different character. There will be a varying error due to irradiation or diffraction, according to the brightness of the sky, and in general a "personal" error differing from that which is common to the star observations of the same observer, and which is eliminated in the process of reduction. It follows then that observations before and after sunset will show a discordance, and observations before and after full moon will show a separate discordance according to the limb which is observed, so that in the course of a month a peculiar set of discordances between observation and ephemeris is found, partly due to the imperfections of the ephemeris, but partly also to the causes just mentioned. To render the observations more comparable, it has been proposed to observe instead of the limb a small "crater" near the apparent centre of the moon as seen from the earth, and at Greenwich, and the Cape, and elsewhere, this has been tried, the crater chosen being known as

Mösting A, for a reason which will appear. The moon's librations in latitude and longitude (since only in the mean is its position relative to the earth invariable, and even then only to the centre of the earth) cause the position of Mösting A to vary with respect to the apparent centre, but this is allowed for by a special ephemeris, given in the *Berliner Jahrbuch*. It must be admitted that there are considerable drawbacks to the method. To make observations of the crater comparable with previous observations of the limb, it is necessary to observe both at the same transit, which is liable to interfere with the consistency and accuracy of both observations, as they must be more or less hurried. Again, the illumination of even a central crater is not constant. It cannot be seen at all in the first or last quarter, and is not always easy to identify, while on some cloudy nights, when a partial transit of the limb is an easy matter, that of the crater is impossible. In any case the crater is not stellar in appearance, so that the error, whose elimination is sought, could not under the very best conditions be entirely abolished, though it might be reduced; under average conditions even this is not probable. Mention of Mösting A brings us to what is called selenography, or the science of charting the moon. The father of this science was Schröter, whom Miss Clerke has called the Herschel of Germany. Before his time Hevelius, Cassini and others had noted some of the salient features of the moon, but until near the end of the eighteenth century, when Schröter settled at Lilienthal and began his lunar and planetary studies, no exhaustive topography, with a view to accounting for the appearance of the moon or detecting possible changes, had been attempted. Schröter discovered the first "rill" on the lunar surface in 1787; about a thousand are now charted; these "rills" are like barren ravines, but it is doubtful whether they are simply dried-up watercourses, or cracks produced in cooling. In 1830 Beer and Mädler began their trigonometrical survey of the lunar surface. Their chart was published in four parts, from 1834 to 1836. Before that Lohrmann had commenced one in twenty-five sections, on the scale of one mètre to a lunar diameter; but having published four sections his sight failed, and his drawings were taken in hand by Dr Schmidt, who became director of the observatory at Athens, and after almost monopolising lunar chartography for many years, published a complete atlas founded on Lohrmann's in 1878. The scale was twice as great as Lohrmann's, two mètres to a diameter, and more than 30,000 craters are marked on it. Schröter had imagined the possibility of lunar inhabitants, "Selenites," and so as a matter of fact had Herschel and others. Beer and Mädler definitely announced that no vestige of change was shown anywhere, and that the moon was dead. Nearly thirty years later Schmidt announced in 1866 that the crater Linné had disappeared, or so greatly changed in appearance to be hardly recognisable. Interest in the lunar surface at once revived, and Schmidt no longer had the field to himself, as had been the case since the announcement of Beer and Mädler. It is curious that



FIRST QUARTER LAST QUARTER

THE MOON

modern observations of Linné agree with those of Schröter, while those of Lohrmann, Mädler, and early ones of Schmidt made it a much larger and more important crater.

But though the continually varying incidence of light on lunar "landscapes" may in certain cases cause strange apparent changes, it is not safe to assume that these careful observers were mistaken, especially as this is not a solitary instance. A similar change on the floor of the "walled plain" Plato has been noted, and also a new crater, Hyginus N., announced. It is considered by W. H. Pickering, whose great work on the moon, from photographs taken principally at Arequipa, is one of the most complete contributions to any branch of astronomy that the new century has yet produced, that the lunar volcanoes are not quite, though very nearly, extinct, and that these changes are real. It can hardly be regarded as certain, but it is a reasonable explanation, if the facts are admitted. Other works on the chartography of the moon have been produced in the intervals between those referred to. Nasmyth and Carpenter published in 1874 an atlas, which has recently seen a new edition; and Neison (Nevill), now director of the Natal Observatory, Durban, brought out in 1876 his great work on the moon, with a revised map, founded on Beer and Mädler, with thousands of additional objects. Photography of the moon was first systematised at Lick Observatory in 1890, another series being commenced at Paris in 1894. Professor Weinek, of Prague, has studied the Lick photographs; and more recently in England, S. A. Saunder, one of the secretaries of the Royal Astronomical Society, has taken up the work of measurement of Paris photographs with a view to a complete selenographic index and atlas, in which, however, he finds great difficulty owing to the alarming want of uniformity in lunar nomenclature. We have referred to Mösting A and Hyginus N, but it must suffice on this subject to indicate that names (*e.g.*, of astronomers as Tycho, Copernicus) have been assigned to conspicuous craters, that other names have been given to larger regions such as the "walled plains," Plato and others; that still larger ones, "Maria," are indicated as Mare Nubium or Mare Serenitatis; and that in the case of a named district with small craters, these are given the "district" name with a suffix, as Mösting A, Hyginus N, and others. It is in the order followed in the suffixes that some inconsistencies occur, while other cases are found of craterlets being assigned to wrong regions, or possibly to more than one, the borders not being very exactly assigned. It is to be hoped that the interest recently shown in this subject will be maintained until a uniform system is consistently followed. Schröter suspected a lunar atmosphere about twenty-nine times thinner than our own; but Bessel, from observations of the suddenness with which stars disappear behind the moon, concluded that the lunar atmosphere, if any, did not refract to any sensible extent. Various estimates of the tenuity admissible under such conditions have been given, Sir John Herschel's limit being $\frac{1}{1980}$ of that of the earth, a very different figure from Schröter's $\frac{1}{29}$.

It was noticed, however, that the diameter of the moon, as deduced from occultation, was less than that obtained by direct measurement. Airy, from Greenwich observations, made the difference amount to four seconds of arc. If this could be attributed to refraction, the effect could be produced by a lunar atmosphere of Herschel's suggested tenuity. But it is not necessary to assume refraction to account for the difference. It is known that when the moon's limb appears best defined for purposes of measurement, it is apparently increased by irradiation, so that the best direct measures are too large. On the other hand, occultations will often, and, therefore, in the mean, give a smaller diameter, as the irregularities in the moon's limb are considerable, and any depression at the point where an occultation takes place has its full effect on the apparent occultation diameter, while it has none on the direct measure, and in the complementary case of a slight protuberance on the limb, both observations are affected. So that from this cause also the "occultation" diameter would appear the smaller. What is called the "eclipse" diameter, on which depends the magnitude and duration of a total solar eclipse, is smaller still, most likely because every depression in the limb has its effect in letting sunlight pass, and so slightly shortening the duration of totality. This difference is readily illustrated by likening the apparent disc of the moon to a solid toothed wheel, the directly measured diameter being rather greater than that of the circle circumscribed about the teeth, the eclipse diameter that of the circle inscribed within the depressions between the teeth, and the occultation diameter between the two, as in general at least some portion of a tooth would be in the path of the star. It is obvious that most occultations take place under conditions which render it difficult, if not impossible, to measure the diameter at all, for only in the case of very bright stars is accurate observation possible at the bright limb, so that either the time of disappearance or of reappearance is liable to error, according as the moon is waning or waxing. On this account, about twenty years ago, it was suggested that advantage should be taken of lunar eclipses for this purpose, for, during an eclipse, not only are both limbs dark, so that complete observations are possible under good conditions, but also the absence of bright moonlight greatly increases the number of stars whose occultation can be observed with accuracy; and there is in general another slight advantage in that the eclipsed limb is more often visible than the unilluminated limb. Hence at more than one total eclipse lists of faint stars liable to occultation during the eclipse have been prepared and circulated among observatories likely to co-operate, with an ephemeris of the times of disappearance and reappearance. In a few instances, what are called "anomalous" occultations have been observed, when stars have either seemed to disappear gradually, or to have been visible through the moon's edge. These cases have been cited as evidence of a lunar atmosphere; but a more probable explanation is that in some cases the star has been visible through a depression in the moon's limb, even after possibly disappearing momentarily behind a "mountain"; and that in other cases the star is really double, and one component has vanished before the other. Advantage has also been taken of lunar eclipses to test the question whether the moon radiates heat on its own account besides reflecting it from the sun. Evidence goes to show that a sensitive thermo-pile of selenium cells exposed to the rays of the moon before and during an eclipse shows a diminution of heat received, not at the commencement of the eclipse, but rather later. This is, however, not conclusive, as it might easily be explained by the slight storage of sun heat at the surface of the moon, which would not dissipate entirely for a short time after the sun's rays were withdrawn, just as the filament of an incandescent electric light glows faintly for a fraction of a second after the current is cut off. This particular branch of lunar investigation is under the special care of the Earl of Rosse, whose father built the great Parsonstown reflector of 6 feet aperture, which is still the largest in the world, though its reflecting efficiency is much impaired by deterioration of the mirror.

The evidence of the spectroscope shows that the light of the moon is simply reflected sunlight, the only difference being the faintness of the spectrum; by Huggins, some forty years ago, the spectrum of a star approaching occultation was watched to see if any differential absorption by a lunar atmosphere could be observed, but the whole range of lines vanished simultaneously.

# CHAPTER XXI

## THE EARTH

IT is sometimes assumed that the earth itself should be left to the geologist on the one hand, or the geographer on the other. But the astronomer cannot afford to neglect the earth from either point of view. Geologists allege a glacial period, stating definitely that the arctic circle must once have been as far south as Yorkshire, and astronomy has been called upon to "state a case." The ordinary cause of a shift in the arctic circle is of course a change in the obliquity of the ecliptic, which is slowly diminishing. But Laplace's investigation has assigned quite a small limit to this change, which is in the nature of an oscillation, and this explanation was abandoned as insufficient. Sir John Herschel suggested changes in the eccentricity of the earth's orbit as another possible explanation, Lagrange having indicated an oscillatory change, subsequently established by Le Verrier, in the shape of the earth's orbit, (the mean distance being invariable), from an ellipse to a circle, or nearly so. This formed the basis of Croll's astronomical theory of an Ice-age, about which, since it first appeared in 1864, much has been written. The reconciliation of the two sciences on this question has, however, been indefinitely postponed, and we need not dwell on it further. Neither need we pay much attention, from the astronomical standpoint, to the question of what is at the centre of the earth. Halley's idea of a solid nucleus is gaining ground steadily, but it was not suggested as an astronomical speculation, but in order to account for the observed difference between magnetic and true north, his notion being that the magnetic poles indicated the rotation axis of an inner solid. And yet in another form this very question of the earth's interior has become of great practical interest to astronomers, though it is not the question of solidity but rather of rigidity that supplies the interest. It was long the custom to consider the earth in problems of dynamics as a perfectly rigid body. The small value of the observed precession and nutation were strong evidence that the earth's crust is not a thin shell, as otherwise it would respond in a much greater degree to the luni-solar attraction on the protuberant part round the equator; and though this argument, advanced by Hopkins in 1839, led to some controversy, it being urged by Delaunay that the internal fluid being viscous and the motion slow, no such increased effect would be expected, Hopkins' conclusion as to the earth's external solidity received confirmation from another direction, the theory of tides. One of Lord Kelvin's numerous contributions to science was the deduction that, since tides are obviously perceptible, they cannot be shared in anything like an equal degree by land and water, as they would be if the earth were not rigid to a considerable extent. Carrying out a series of tidal observations suggested by him in 1868 (he was then Sir W. Thomson), Professor Darwin, from the analysis of a large quantity of data extending over more than thirty years, announced to the British Association in 1882 that the earth's effective rigidity was at least as great as that of steel.

Soon after this Küstner, at Berlin, detected an apparent slight variation in the latitude, and careful observations in selected places, widely differing in longitude, gave such confirmation that, in 1891 S. C. Chandler was able to reduce them to the concrete form of an assertion that the earth's pole approximately describes a sort of circle of about twenty yards in diameter every fourteen months. Newcomb suggested as a physical explanation that the earth's axis of figure, about which it is bound to be rotating instantaneously, must be continually changing, owing to the successive piling up and melting away of masses of ice and snow, and even the unequal motion of the surrounding air. "Rigid" dynamics being applied to the problem pronounced the theoretical period of this particular oscillation to be 306 days, or say, ten months. For an earth of steel the time was found to be 441 days, or about half a month longer than Chandler's 428 days. It may therefore be concluded that the earth's effective rigidity is rather greater than that of steel, a result affording independent confirmation of that of Professor Darwin.

Since the adoption of Chandler's result, it has been necessary to allow for the variation of latitude in exact astronomical measurements, and causes and variations of the period have been diligently sought. The amount is not a constant from period to period since the supposed causes are variable, one underlying factor in all such meteorological conditions being the solar radiation, which is known to be subject to variation and is suspected to show a connection with the sun-spot period. A small annual variation of latitude has been announced by a Japanese astronomer, Professor Kimura, who describes it as a shifting "up" and "down" the earth's axis of its *centre* of gravity, which at once suggests a probable cause in the accumulation of ice alternately at the north pole and the south, this being an annual phenomenon which might be expected to produce just such an effect. It has, however, been confidently stated that the effect of this cause would be much smaller than the "Kimura phenomenon," and, moreover, in the opposite direction. But the precise effect of the accumulation of ice, with the accompanying redistribution of water, might easily be different in quantity and sign from what might at first sight be assumed; and the behaviour of the air at the poles is not known, so we may regard the matter as still open to speculation. It will be noticed that we have worked round to the geographical side of the subject, but on that astronomers have a strong prescriptive right. The first geographers were astronomers, Ptolemy for instance, and all fundamental "large scale" geography, such as the determination of the earth's size and shape by measurement of meridian arcs, has been for centuries in the hands of astronomers. In fact, the fundamental problems in all extensive surveys, as well as in navigation or oceanography, are the determination of the latitude, and more important still, because more difficult, of the longitude, that is, the time, whose accurate determination is so important a branch of the work of a national observatory.

Geodesy, on the large scale, is a matter of great importance, and the International Geodetic Association, with headquarters in Berlin, has now secured the support of nearly all civilised countries. Much has been done since the early days of the French Revolution, when the metric system was first introduced, the unit of length, the mètre, being defined as the $\frac{1}{10000000}$ part of the arc from the pole to the equator. This arc has, of course, never been measured, but meridian arcs in various parts of the world have been measured from time to time, in Lapland, in Peru, in Spitzbergen, in France, the greatest scheme being that still in contemplation, and partly executed, through the whole length of the African Continent, with possible extension by way of Palestine to join the Russian arc. We cannot enter into the details of trigonometrical surveys, the special wires required for measuring, the various standard base-lines, and the many other precautions necessary in such a work as the great Indian survey, that of South Africa now in progress, the continual work of the U.S. Coast and Geodetic Survey, and others. It may be mentioned that, in the matter of ordnance surveys, our own country is behind many other nations, but these are outside our purpose. The determination of differences of longitude between fixed stations is certainly a matter of astronomy; at one time these were dependent on chronometers transported from one place to the other, the error and rate of the chronometers being determined by astronomical observations. The invention of the electric telegraph superseded this method, and led to the practice of observing a set list of stars on the meridian at each station on the same nights, comparing the clocks by electric signals through the telegraphic system, and subsequently interchanging observers at the two stations, in order to eliminate any systematic personality. In this way the difference of longitude, Greenwich-Paris, has been determined again and again, with a small difference between the English and French determinations; the difference, Greenwich-Montreal, once in recent years, by means of intermediate stations at Canso (Nova Scotia) and Waterville (Ireland), to divide the distance into what are practically two land sections and the Atlantic cable, though of course the Greenwich-Waterville section also includes a submarine cable. A great arc of longitude has been also measured by the officers of the Indian survey to connect their system with the European, and more important than all, the labours of the German bureau, under Professor Albrecht, have practically completed a ring of determinations round the earth by way of Japan and North America, all made with exactly similar instruments. It is in connection with this work that the Repsold self-recording transit-micrometer, referred to in a previous chapter, has been used to eliminate nearly all personality. The latest development, which has not progressed much further than the experimental stage, is the adoption of wireless telegraphy for the interchange of signals, a method by which many small errors, due to the system of wires, batteries, and relays, may be eliminated, and the probable error of the resulting determination considerably reduced, if, indeed, the word "considerably" may fairly be applied to the very small residuals already shown.[1]

Of late years a simplification in the change of civil time, from one country to another, has found more general adoption. It was long urged that all countries should adopt a universal central meridian, and that the difference between the standard time of two countries, instead of being so many hours, minutes, seconds, and decimals of a second, as determined by the exact difference of longitude of the national observatories, should be in general an exact number of hours, or possibly half-hours, chosen so as not to throw noon far from twelve o'clock over the greater part of either country. The principle being admitted, great controversy raged as to the choice of a central or zero meridian, the almost prescriptive right of the Greenwich meridian meeting considerable opposition in some quarters, on the plea that it is not central for any extensive land area, on which ground, and also to avoid international jealousy, the meridian of Jerusalem was proposed, among other alternatives. With the strong support of America, Greenwich obtained a large majority of the votes at a special International Conference, and by the present time nearly all civilised countries have fallen into line, so that Spain, for instance, has adopted Greenwich time, and Eastern China eight hours fast on Greenwich.

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[1] The error in the "girdle of the earth" from Greenwich to Greenwich, viâ India, Australia, and Canada, is less than a fifth of a second of time in the twenty-four hours, or about one part in half a million.

The measurement of great arcs is not the only important branch of geodesy, which also concerns itself with the variation of gravity, due either to local disturbance, such as the vicinity of mountains, or to the latitude, the force on the pendulum varying as it is carried further from the equator, owing to the fact that the earth is flattened towards the poles. The amount of the compression has been determined in this and other ways, and is not far from $\frac{1}{300}$. Pendulum observations, to determine the actual disturbance of gravity caused by a mountain, in order to infer the mean density of the earth have yielded a result about five and a half times that of water, a value close to that which Newton suggested. As most of the surface rocks are much lighter than this, it seems likely that the centre of the earth is composed of materials either in themselves heavier or else subject to enormous pressure.

There remains the earth's atmosphere, which has an intimate connection with astronomical investigations, if only for the trouble entailed in eliminating its various effects, among which may be noted variable refraction, which affects most measures of celestial objects and angles; air currents which interfere with good definition, on account of which it is becoming increasingly important to choose a suitable "location" for a new observatory; and absorption, which provides the main problem to be dealt with by the bolometer. Almost the only definite work (other than elimination of disturbing effects) astronomy finds in the air itself seems to be the identification of telluric lines (even this having for its principal object nothing more than their elimination from solar and other spectrograms) and the investigation of the Aurora. The main interest of the atmosphere is certainly with meteorology and cosmical physics. We must pass on to regions more exclusively astronomical.

# CHAPTER XXII

## THE INTERIOR PLANETS

MODERN advances in planetary observation are confined to comparatively few channels, and the most attractive of these, in the case at any rate of the larger planets, has been the rotation. Schröter worked for thirty-four years at Lilienthal before the catastrophe of 1813, when French troops pillaged and destroyed the place and ruined the observatory, many of Schröter's manuscripts perishing in the conflagration of the Government offices (Schröter was chief magistrate of the district). During this time he observed the surfaces of the planets, as well as of the moon, with a care and perseverance never before devoted to them. He inferred the existence of a fairly dense atmosphere on Mercury from the relative faintness of the "terminator," the boundary of the illuminated portion of the planet, and also from an appearance like a bright halo round the disc when seen in transit across that of the sun, giving a shaded rim to the dark disc. At different transits this appearance has been seen by some observers and absolutely denied by others, so that it is generally considered to be an optical phenomenon. Photometric observations of the partial phases have tended to show that light is reflected similarly, and to exactly the same proportion, from the surface of Mercury and from that of the moon, from which we may conclude that Mercury has no appreciable atmosphere.

Schröter noted an apparent blunting of one horn of the crescent of Mercury which he assumed to be caused by a mountain; from repeated observations of the times of similar appearances he inferred a rotation period of rather more than twenty-four hours. A similar, though slightly shorter, period was deduced from observations of some rather uncertain markings. Several observers, including Trouvelot and Denning, noted features similar to those described by Schröter, but the next astronomer to devote much time to the subject was Schiaparelli at Milan. By working in daylight he considerably increased the time during which the planet could be continuously watched, and also was able to observe it under much better conditions high up in the sky. The final conclusion, after a long series of observations, was that Mercury has reached the stage of development as a planet that the moon has as a satellite, and turns on its axis in the same time in which it revolves round the sun, about 88 days. So that as the moon, except for libration, always turns the same face to the earth, so Mercury always turns the same face to the sun, except that owing to the eccentricity of the orbit Mercury's librations are much greater than those of the moon, so that only about three-eighths of its surface is never exposed to the sun. Schiaparelli also observed markings somewhat in the nature of colour contrasts, whose only occasional visibility led him to assume an atmosphere, but Lowell, in the clear air of Arizona, has since verified Schiaparelli's conclusions as to the rotation, but carries the analogy with the moon still further by pronouncing Mercury an airless, dead planet, with a surface cracked in cooling untold ages ago.

Although we have begun this chapter with Mercury as the planet nearest the sun, we must not forget the possibility that this may not really be the case. The anomalous motion of the perihelion of Mercury has been a stumbling-block to the universal satisfactoriness of the law of gravitation, and one suggestion, worked out by Le Verrier on analogous lines to the analysis by which he had predicted Neptune, was that there was a planet nearer the sun than Mercury, whose action would account for the anomaly.

Six months before the announcement of this hypothetical planet to the Academy of Sciences, a provincial French physician named Lescarbault had at last succeeded, after years of patient watching, in seeing a round object slowly traversing the sun's disc. Hoping to see it again, he said nothing until after the publication of Le Verrier's result, but then, in spite of snubbing and severe cross-examination from Le Verrier, who hurried down to see him as soon as he heard of the supposed discovery, he succeeded in convincing the great man of the accuracy of his observation, and an orbit was approximately computed for the alleged new planet, to which the name Vulcan was assigned. It has never been seen since, though similar appearances were collected and investigated, and transits predicted. One such appearance in 1876 was proved to be an ordinary sun-spot, without penumbra, and the next announcement was at the eclipse of 1878, when, as already noted, two American professors, Watson and Swift, at different stations, declared they saw a planetary object near the eclipsed sun, in fact two such objects. Analysis proved that none of these fitted the calculated orbit of Vulcan; and that, so far as the observations went, they did not refer to the same two objects, unless these were two not very bright stars in Cancer. Those two stars, it is now generally assumed, they actually were, and it is also assumed that excitement rendered the identification doubtful by making the observations unusually bad for such qualified observers, each famous for astronomical discoveries. At no subsequent eclipse has any such object been announced, though much time has been devoted to the search for it by means of charts showing every star in the neighbourhood of the sun which could possibly be visible in the darkest eclipse.

The planet next after Mercury in nearness to the sun is Venus, whose transits, as we have seen, were considered of such importance in the problem of the solar parallax. Venus is considered to be much like the earth, not differing greatly in size and showing less equivocal traces of atmosphere than have been noted in the case of Mercury. Moreover, its rotation period has been by many observers from the 17th century to the present day considered to be nearly the same as that of the earth. Thousands of observations have pointed to a value just over 23¼ hours. Schiaparelli, however, noted that certain bright spots apparently remained always at the same distance from the terminator, and hence concluded that like Mercury and the moon, Venus also keeps the same face always turned to its primary, or in other words its rotation and revolution periods are identical. This gives a rotation period of 225 days, very different from the results of Cassini, Schröter, De Vico and others. But the case was not considered so certain as that of Mercury. It was at once suggested that a spot appearing at the same place on consecutive days did not negative the possibility of a whole rotation in the interval, and though this particular objection has been met by a pair of practically identical drawings made on the same day in 1877 by Schiaparelli at Milan and Holden at Washington, with an interval in actual time of eight hours, yet there are not wanting modern observations in support of the quick rotation period. Such well-known names as Perrotin (Nice), Tacchini (Rome), and Lowell are on the side of Schiaparelli; but on the other side may be found almost equally careful observers, in support of whom may be cited Bélopolsky's attempt to test the matter spectroscopically in 1900, which apparently confirmed the short rotation period. But more recently, in 1903, a similar method in the hands of V. M. Slipher, working at Lowell's Observatory, Flagstaff, Arizona, gave no evidence of a quick rotation. It was objected that this was only negative evidence, but the same method applied to Mars gave a result within five per cent. of the truth as known from other methods.

At the risk of repetition it is perhaps advisable to indicate briefly in this place the underlying principle in the spectroscopic method of determining rotation. Light from the two opposite limbs of the planet is examined simultaneously with the spectroscope. Relatively one set of rays is approaching the observer with the linear rotation velocity of the planet, the other receding with the same velocity. Hence the displacement between the two corresponds to twice that linear velocity. The quicker the angular rotation, and the larger the planet, the greater will be the displacement and the easier and more reliable will be the observation. Mars, however, is smaller than Venus, and his rotation period a little slower than that demanded by the opponents of Schiaparelli. It is therefore difficult to evade the argument that if they were right Slipher's spectroscope *must* have proved it, hence the fact that it did not is positive evidence that they are wrong. It seems safe, therefore, to assume that the rotation period is 225 days, no intermediate value between that and approximately one day having met any support whatever.

Long-continued observations of the appearance of Venus have rendered it fairly certain that Venus has an atmosphere of considerable extent. Effects have constantly been noted that can only reasonably be attributed to refraction of the sun's rays beyond the terminator, and the bright limb is frequently greater than a semicircle, and occasionally is seen right round the planet, which could hardly be the case unless distinctly more than half the planet were illuminated. The dull light occasionally seen on the dark portion of the disc cannot be explained by earthshine, as is the case with the corresponding lunar phenomenon. Much of the evidence on this subject is to be found in the various transit of Venus observations, where the effect of an atmosphere is very marked. And yet there is evidence that considerable irregularities are also seen on the surface. We need not accept Schröter's mountain of 27 miles in height, but careful observers such as De Vico and Denning have confirmed the existence of formations similar to those on the moon. It appears then that there is an atmosphere which twilight observations indicate as being of considerable depth, and that we can nevertheless see mountain formations through it. On the other hand, comparison with Mercury, considered as an airless planet, shows Venus to possess so high a light-reflecting power or *albedo* that it has been suggested that no solid body could be so bright, and that what is seen is an atmosphere charged with clouds, or possibly snow. It seems evident that more work remains to be done to enable a satisfactory decision to be made. Can it be that the main portion of the visible surface is simply a cloud world veiling the planet beneath except for a few lofty snow-covered peaks occasionally rising above the clouds? It is true that maps of Venus have been produced by Lowell, for instance, showing markings of an indefinite character, of a spoke-like formation, but even Lowell is inclined to consider them as not real surface markings but as optical effects, so that it cannot be said that they absolutely negative the cloud-mask idea.

The spectroscope as applied to the light of Venus furnishes very slight evidence of any absorption of the sun's light by water-vapour or anything else, so that there appears to be little doubt that most of the light at any rate which we receive has not penetrated far into a dense atmosphere. These observations are not always accordant, some well-known spectroscopists having found distinct indications of water-vapour, others practically none. The absorption is perhaps a more variable quantity than anything of the kind in our own atmosphere, and in any case it seems certain that conditions on Venus are not so similar to those on the earth as has sometimes been claimed.

# CHAPTER XXIII

## MARS

THE planets between us and the sun are by reason of their position less favourably placed for observation from the earth. We now turn to the outer planets, and are almost bound to commence with Mars, though, as we have already seen, Mars is not quite the nearest at all times. The value of Mars in the history of astronomy is very great. It is impossible to conjecture how much longer the world would have had to wait for the laws of motion enunciated by Kepler but for the considerable eccentricity of the orbit of Mars, which was sufficient to preclude the possibility of a circular orbit. When Venus is nearest the earth it is only the dark hemisphere that is turned towards us, but with Mars nearest it is the bright hemisphere that is seen, so that long spells of observation at night are frequently possible. It was once considered that the red colour of Mars denoted a dense atmosphere, corresponding to the foggy conditions under which the sun appears red to us; but this has long since been disproved. Observations of Mars and comparison stars for determination of solar parallax have been already noted, and we may now confine our attention to questions concerning Mars alone, which can in many cases, owing to the favourable conditions and the consequent concentration on it of a large number of observers, be answered with more definiteness than is the case with any other planet.

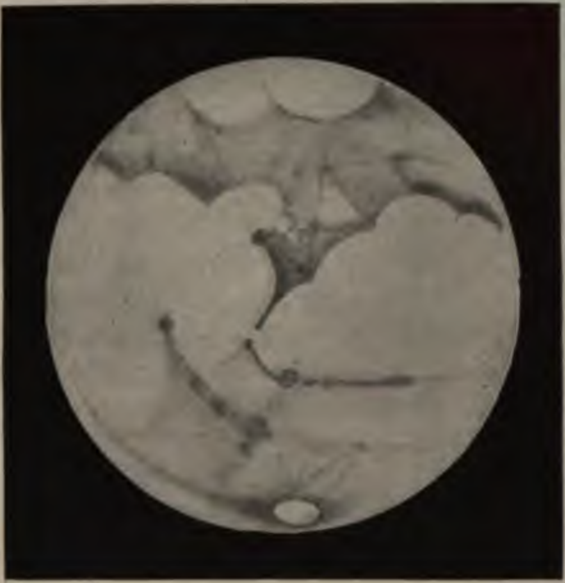
There is none of the doubt that at one time existed in the case of Mercury, and to some extent still exists in that of Venus, as to whether we can see permanent markings from which to deduce the rotation period. This was found with considerable accuracy to be $24\frac{2}{3}$ hours by Hooke and Cassini in the 17th century, and some fifty years later Maraldi noted the "polar caps," subsequently explained by Herschel to be actual frozen precipitations, inasmuch as they alternately increased and diminished in extent with the progress of the Martian seasons. It is true that Schröter and some of his contemporaries denied the reality of the surface markings, alleging that they were merely cloud effects, but the steady improvement of optical power in the 19th century has enabled successive observers to indicate markings with such confidence that identifications have been made with the very oldest markings on record, and maps differing thirty years or more in date show practically the same features.

With a range of observations extending beyond two centuries, even an error of a tenth of a second in the received rotation period is inadmissible. Professor Bakhuyzen, of Leyden, from a comparison of observations from Huyghens to Schiaparelli, gives the period as 24h. 37m. 22.66s., an error of one-tenth of a second, which corresponds to more than two hours in the interval between the first and last observations, being practically impossible.

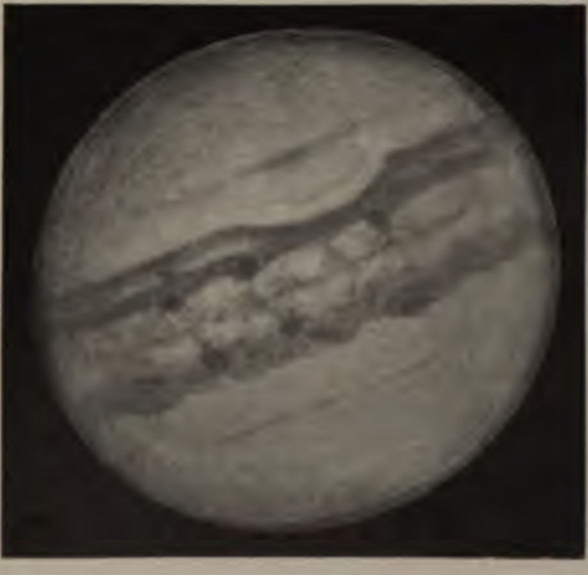
Many independent pieces of evidence accumulated in the last century to overthrow the notion that the redness of Mars was due to atmospheric absorption. A star occulted in 1822 was observed by Sir J. Herschel's friend South to disappear sharply, and soon afterwards Herschel ascribed the colour to soil. Another argument adduced by Dawes was that the red colour was deeper in the centre instead of at the edges. Yet another was the whiteness of the "polar caps," adduced by Huggins. Theory, moreover, based on the relative effect of gravitation, points to the probability of Mars, a body much smaller than the earth, having an atmosphere very little more than one-seventh as dense as ours.

Perhaps the strongest argument of all is the fact that, in general, details of the surface of Mars can be seen, though clouds or vapours of a similar kind are often noted also. Were the atmosphere of Mars as dense as ours, it is known, from Langley's bolometric observations, that 40 per cent. of the sun's vertical rays would not reach the surface of Mars, that even white sandstone would not reflect a quarter of the remainder, and that probably another 40 per cent. of what was left would be lost in its passage out again; so probably less than 10 per cent of the light would reach the earth's atmosphere. At the same time the light reflected from the cloudy atmosphere of Mars would be much brighter in proportion. So it is practically certain that no surface detail of Mars would be seen except in the vaguest manner, but for the great tenuity of its atmosphere compared with ours.

The spectroscope in recent years shows an almost total absence of water absorption, though traces have been noted by Huggins and Vogel. This feature, though partly to be expected on account of the smallness of absorption due to so slight an atmosphere, is also regarded as evidence of a considerable lack of water on Mars altogether. The melting of the "polar caps," which sometimes is complete, a very different state of things to that which obtains on the earth, seems to point to a temperature far higher than the theoretical mean temperature (some 30° below zero Fahrenheit, according to Christiansen). But it has been freely surmised that the "polar caps" are not of snow, but of solid carbonic acid, with a much lower melting point; if so, not only would the one anomaly be plausibly explained, but also the similar one of the apparent absence of "frost" beyond the polar regions at all times, for once more we can appeal to the investigators of solar radiation, of whom Ångström finds a very high value for the heat-absorption of carbonic acid gas. It would seem possible that the so-called "theoretical" temperature, deduced from the distance and *albedo* of Mars, is absolutely unreliable, and that, owing to the relative preponderance of carbonic acid instead of water vapour, Mars may retain comparatively more heat than the earth. But we shall note further suggestions bearing on the comparative dryness of the planet. Among the many careful delineators of the features discernible on the very small disc, which only at "favourable" oppositions, occurring about once in fifteen years, reaches an apparent diameter nearly one-seventieth part of that of the sun or moon, the first associated with an important discovery in more recent times was Schiaparelli, who in 1877 discovered what have ever since been called "canals" (the Italian word really means "channels"), a network of more or less straight divisions, extending from "sea" to "sea" across what had been regarded as "continents," but which might now be considered "islands." The erroneous use of the word "canal" gave rise to conjecture that the divisions were actual irrigation canals by which the melting of the "polar caps" was made available for the spread of vegetation. The size, however, some of them being three or four thousand miles long and about sixty miles wide, seemed conclusive against this hypothesis. In the winter of 1881-82 the planet, though further off, was much higher in the sky for observers in the northern hemisphere, and Schiaparelli made the astonishing discovery that many of the "canals" were double, a companion running parallel at a distance varying from 200 to 400 miles. Much ingenuity has been applied to the explanation of this phenomenon, either as an illusion or a reality. It has been attributed to refraction or some kindred optical cause. Professor Lowell has written a celebrated book on Mars strongly supporting the theory of the "canals" being artificial irrigation works; and maintaining, moreover, that the reddish and grey-green regions, so far from being simply land and water, or "continents" and "seas," are land only, divided into deserts and tracts of vegetation; to the theory that the "seas" are not water comes supporting evidence from Professor Barnard with the great Lick telescope, his observations, in great detail, being incompatible with the idea of a large expanse of water. The favourable opposition of 1892 was principally famous for the great interest evoked by the fantastic idea eagerly taken up in the press that certain bright clouds noted in the atmosphere of Mars were actual "signals" from Mars to the earth. Careful observations, especially at Arequipa, in the southern hemisphere, where the "favourable" oppositions are most deserving of their name, confirmed the previous impression that the outlines are only in the main permanent, but that changes considered seasonal do considerably vary the contour of the various districts, so that either there are extensive inundations or luxuriant tropical growth, according as the colour be taken to denote sea or vegetation. It is probable that the opposition of 1907 will see a renewal of interest in those matters, especially in the southern hemisphere. The Cape is now supplied with a fine instrument, and there is talk of one being provided for the Transvaal, though there is hardly time for that before the opposition. Quite recently another book from Professor Lowell emphasises still more strongly his conviction that the observed phenomena demand polar caps of snow, not carbonic acid, inasmuch as he maintains the visibility of a fringe of liquid during the melting, whereas ordinarily carbonic acid goes straight from solid to gaseous form. He insists also that everything apparently anomalous in the drawings made at his observatory is a natural consequence of the shortage of water on the planet, which is in a far more advanced stage of development than the earth (which is also said to show symptoms of a gradual "drying-up"), so that on the one hand necessity has driven the assumed "Martians" to construct elaborate irrigation works in order to utilise to the full the scanty supply of water, and on the other the absence of mountains and the greatly diminished effect of gravity have enormously lessened the labour involved in the necessary trenching. When we remember the tenuity of the Martian atmosphere we might fairly object to the assumption that the supposed inhabitants would get the full benefit of the economy of power alleged, as their vital energy, an important factor in the question, would be probably low. In fact, so far as the argument depends on terrestrial and human analogy the assumptions involved are so great that Professor Lowell's very plausible explanation can hardly be called convincing. This is especially the case since practically the whole of the elaboration of the details, the close network of markings crossing not only the "deserts" but also the "vegetation," the "oases" at the points of intersection, and other phenomena dealt with at length in his book, depend on drawings made at his own observatory by his own staff; and it is difficult for those who have looked in vain with different instruments under different conditions to admit the full deductions at once to be drawn from their failure and his success. Admitting his evidence, his deductions as to intelligent beings on Mars, who can not only construct thousands of miles, in fact hundreds of thousands of miles, of great canals at least a mile wide, but can also force water to fill them against its natural inclination, are scarcely, if at all, credible. It is, therefore, easier to doubt the reality of the evidence, elsewhere unsupported, except in some of the more conspicuous features, though this attitude is more human than logical. It is well known that a trained observer, who knows by experience exactly what he is looking for, will see with comparative ease what an average person entirely fails to distinguish; but it is also beyond question that observers of supposed trustworthiness have recorded things as seen which have had no real existence, but which they expected to see. In other words personal bias counts for something, and whether the observation be made by the man who believes or by one of his staff who is told what to expect, it will be regarded with scepticism by the man who does not believe. And on this subject we may note two interesting experiments. Some time ago at Greenwich Hospital School, in accordance with a suggestion made by E. W. Maunder of the Royal Observatory, a number of boys at different distances were set to copy what they could see of a design similar to a vague map of Mars without any



MARS



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lines joining the salient features, none of the boys having any previous knowledge of the subject, and produced a striking result. Those near the board practically reproduced the design; those far off only imperfectly did the same, but some at intermediate distances did actually put in, as visible to them, lines resembling the famous "markings" on Mars. This experiment, though as Professor Lowell says, it does not prove him to be mistaken, at any rate certainly shows how he might be mistaken, and has shaken the convictions of some planetary observers. The other experiment was carried out by Mr Lampland, one of Lowell's assistants, in 1905, and was a successful attempt to photograph a "doubled canal." It unfortunately happens that the particular canal photographed, being naturally the widest "pair," was generally regarded by Lowell himself as not a normal case, and certainly the distance between the two parallel lines, wide enough to enclose Great Britain between them, seems decidedly against their close association.

On such a subject we may well wait for further illumination before attempting to dogmatise.

The opposition of 1877 was also marked by the discovery, made by Asaph Hall with the great Washington Equatorial, of two small satellites attending Mars. It is curious that at least two writers of romance, Cyrano de Bergerac and Dean Swift, should have predicted this many years before, the latter giving an astonishingly accurate guess as to their motions; so nearly accurate indeed as to raise in some minds a suggestion as to the possibility of the Dean of St Patrick's having actually observed the satellites on the sly and computed the motion of one of them. The suggestion is hopelessly impossible, having regard to the poor telescopic power then available, even granting that Swift's peculiar temperament might have accounted for his deliberate suppression of the discovery, or rather for his publishing it in such a way that no one would suspect the truth, though he would at any time be able to prove priority—a device similar to the anagrams of Galileo and others.

These satellites, called Deimos and Phobos, are very small, probably not more than ten miles in diameter ; and their motions, to anyone familiar with that of our own moon, quite disconcerting. Deimos, the outer one, revolves about Mars in rather more than thirty hours, so that it moves slowly across the sky, taking more than two Martian days from rising to setting. Phobos, meanwhile with a period of less than eight hours, hurries round in the opposite direction, rising in the west about twice a day. Both being so small, however, they are inconspicuous even from the surface of Mars, and Phobos is too near the planet to be even visible from the polar regions. To the hypothetical Martian astronomers these little objects might have been of untold value in simplifying the problems and testing the theorems of celestial mechanics. An absurd rumour, probably due to an imaginative journalist, that these satellites showed phases as they revolved *round Mars* may have aggravated the scepticism in some quarters towards Lowell Observatory announcements.

# CHAPTER XXIV

## MINOR PLANETS

TO the rapidly increasing band of minor planets or asteroids we cannot devote much space. We have seen how slowly the number crept up at first, with frequent intervals of many years between successive discoveries. Since 1846 no year has been fruitless in this direction, and a great impetus was given to the rate of increase by the application of photography to the search about 1892. In former years the patient, arduous work of discovering a very few was deemed worthy of recognition by the award of a gold medal. It is far otherwise with the taking of a photograph in which a faint streak instead of a star-like image at once betrays a moving object, the difficulty now not being the recognition of the planetary nature of the object, but the question whether it is new or identical with one of the many hundreds already noted. Joel H. Metcalf, of Taunton, Massachusetts, inverts the method in general use, and allows an average motion to the plate, so that the stars trail, claiming thus to be able to photograph fainter planets than would be the case if the planets had to trail. When the total number known was small, all were kept fairly under observation at successive oppositions and elements of orbit soon computed and corrected; and even when twenty were discovered in one year computation was not far behind, so that rediscoveries were easily identified, and the permanent number, indicating order of discovery, was soon assigned to each new object, speedily in most cases followed by the selection of a name by the discoverer, the names as a rule being feminine ones from classical mythology. It will be easily seen, however, that while under the old routine it was almost impossible to recognise the planetary nature of an object until sufficient observations had been secured to determine its orbit, the new method reversed the order of things, and so a new convention was adopted. Any planetary object not immediately recognised from a current ephemeris was assigned a provisional letter in addition to the year of discovery, thus 1892 A, 1892 B, and so on; the intention being to begin the alphabet again each year, and to keep the provisional designation until an organised committee was satisfied with the orbit, and convinced of the "newness" of the planet, when a permanent number would be assigned. As no fewer than twenty-seven were discovered in the first year of the new scheme, which was only in effect for part of the year, it was necessary to modify it, and after once restarting the alphabet it was agreed to prefix each letter in turn to successive alphabets, beginning them without reference to the year, so that AZ was immediately followed by BA, and so on. It is almost certain that ZZ will be claimed during the current year, and will be followed by 1907 A, 1907 B, and so on, according to the system of 1893. This will not, of course, mean that 702 new planets have been discovered since the adoption of the present convention in 1893, for the usual announcement in the *Astronomische Nachrichten*, following one of the periodical meetings of the committee, is divided into at least three headings;—permanent numbers have been assigned as follows, certain others have been identified as follows, and again certain others have had no number assigned, having been insufficiently observed;[1] in each class the provisional letters are of course given. The last provisional letters claimed in 1906 were XH, and the last permanent number assigned was (601).[2] The naming lags behind, and conventions are increasingly difficult to satisfy, so that we find a few masculine names, as Endymion; some names from Indian or Peruvian mythology, as Siwa or Cava; some names, perhaps chosen on account of their occurrence in Northern saga or Teutonic legend, as Hedwig or Undine; others frankly in common use, as May and Charlotte; geographical or quasi-geographical, as Chicago, Bavaria or Pittsburgia; or indicating the mode of discovery, as Photographica and Stereoscopia. Professor Wolf recently named a batch of forty-one of his discoveries at once, but there are still many yet unprovided with names. One masculine name, Eros, to which reference has been made in connection with the solar parallax, is defended on the ground that the asteroids are defined as lying between the orbits of Mars and Jupiter, whereas Eros revolves almost entirely within that of Mars; therefore, not conforming to the regulations for asteroids, it need not be bound by the conventions in regard to name.

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[1] In this class must be included for the present 1903 LXa, the only planet ever discovered at Greenwich. The discovery was made in 1906 in measuring photographs taken in 1903, and the provisional letters LXa indicate that the first photograph was taken between the announcements of 1903 LX and 1903 LY.

[2] As these sheets are passing through the press, the letters have reached ZS, but the numbering has not advanced.

The discoverers are far less numerous than the asteroids themselves, some discoverers, as Max Wolf at Heidelberg and Charlois of Nice, claiming three-figure totals, practically all photographic; and some of the older observers, before the application of photography, numbered their discoveries by the score. The new method must have come with a terrible shock to some who had laboured in the field with indomitable patience, and found themselves supplanted, like the hand-loom weavers, by the introduction of the power-loom. Formerly too, men like Dr R. Luther of Düsseldorf spent their time in calculating ephemerides for themselves. This work is now almost wholly done by the Berlin Recheninstitut, which computes orbits from observations, corrects them for planetary perturbations, and pubblishes ephemerides for rediscovery.

The most striking discovery in this fruitful field since Dr Witt of the Urania Observatory, Berlin, achieved his first[1] planetary discovery, Eros, in 1898, was of 1906 TG (= 588).

Long ago Lagrange calculated that if two bodies moving round the sun at the same mean distance from it, were also at the same approximate distance from each other, and projected under certain conditions so as to form with the sun an equilateral triangle, this arrangement would be permanent. Such a case is approximately provided by (588), which is at about the same mean distance as Jupiter, and though not fulfilling all the conditions forms with it and the sun a system very similar to that conjectured by Lagrange.

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[1] It was on this account suggested that the planet should be named Brevitas, as being the sole (discovery) of Witt.

This discovery, with that of Eros, extends the minor planet zone at least the whole way from the orbit of Mars to that of Jupiter, with hundreds of interlacing but not intersecting paths. The space is not evenly filled up, however, for most of the mean distances are near that of the conjectured planet filling the gap in Bode's law between those of Mars and Jupiter; and besides, at certain points, where the periodic times would be a simple fraction ($\frac{1}{2}$, $\frac{2}{5}$, $\frac{3}{7}$ and so on) of that of Jupiter, there are practically no planets yet discovered. Professor Kirkwood argued that this feature, which he noted before the number of asteroids reached 100, was due to the cumulative effect of Jupiter impulses at the regular intervals provided by the simple commensurability of the motions (just as large vibrations can be set up in a suspended magnet by regular presentations of a small piece of steel at intervals equal to one or more complete oscillations of the magnet), which he suggested would in course of time draw them far enough from their original courses to allow of the interference of neighbouring planetoids and thus modify their elements. Whether this theory or some other, such as that of De Freycinet, who argued that Jupiter prevented rather than abolished the existence of such "commensurable" planets, it seems certain that the gaps are intimately connected with the giant planet.

The total mass of the minor planets is probably many times smaller than that of the moon, the estimates having decreased steadily from Le Verrier's suggested limit of a quarter of the earth's mass down to less than a three-thousandth of it. The largest of them, now known to be Ceres, the first discovered, is not 500 miles in diameter; Vesta, the brightest, having a diameter only half as great. Many cannot be much more than ten miles across, and it would seem likely that very many more smaller still are too faint to be discovered with our present appliances. The size of Deimos and Phobos might suggest that they had once belonged to the group of asteroids, but no other evidence is likely to be forthcoming, and speculation is of no value in such a case. A few of the asteroids are observed to fluctuate in brightness (Eros among them), an appearance probably due to want of uniformity of surface. They can have no atmosphere, being too small to arrest the escape of gaseous particles.

It must not be supposed that the asteroids Eros and TG are the only ones of special interest. There are many others noted for some peculiarity in addition to Ceres and Vesta, to which reference has been made as the largest and brightest respectively. For instance, Pallas moves in an orbit inclined at an angle of nearly 35 degrees to the ecliptic; (475) Ocllo has the high eccentricity of 0.38 and other abnormal elements: (132) Aethra, whose orbit is to a slight extent within that of Mars, may claim a place in an "agony column" as a missing legatee, the "something to her advantage" being derived from the fact that the discoverer, Professor J. C. Watson, fearing that among the rapidly increasing host of minor planets, his twenty-two discoveries might sink into oblivion, provided in his will for their continued observation and the complete determination of their orbits. Aethra, however, has not been seen since the opposition at which it was discovered, and it is thought possible that perturbation by Mars may have altered the orbit, a point of considerable improbability which was being recently investigated in America.

Some of the planets occur in pairs, so to speak, the best known being (37) Fides and (66) Maia, whose elements are closely similar, except for the direction of the major axis; (97) Clotho and (3) Juno are not quite so accordant; others again differ in more than one element, but are very similar in others, until we reach pairs whose orbits are alike only in shape and size, such as (27) Euterpe and (287) Nephthys.

[Since this chapter was set up in type two new new planets, VY and XM, have joined the TG class, and masculine names have been assigned to all three, TG being called Achilles, VY Patroclus, and XM Hector.]

# CHAPTER XXV

## THE MAJOR PLANETS

IN striking contrast to this collection of solid objects, we come to their next neighbour, Jupiter, with an enormous bulk more than a thousand times as great as that of the earth, but by no means so solid. It is, in fact, considered now as in a stage intermediate between that of the sun and that of the inner more matured planets. Glimpses of an idea of similarity between Jupiter and the sun, and especially in regard to temperature, are to be seen even in a few eighteenth century writers; but soon after the middle of the nineteenth century the idea was revived by Nasmyth, and in 1860 G. P. Bond found the light of Jupiter fourteen times as intense photographically as that of the moon, pointing to the unexpected deduction that it reflected more than 100 per cent. of the incident light, and so must be self-luminous to some extent. This deduction, however, he himself rejected on the ground that his observations were not good enough, but suggested that any light from Jupiter not attributable to reflected sunshine might be auroral in character.

Soon afterwards Zöllner, in 1865, rejecting the suggestions of Herschel and others that the bands and cloud movements on Jupiter were analogous to trade winds, which in a rapidly rotating body like Jupiter would have enormous effect, suggested that this analogy was misleading, since at such a distance the sun's effect in causing trade winds must be only $\frac{1}{27}$th of that on the earth. He deduced that the requisite heat must come from within, and that Jupiter must be still a hot and almost gaseous body without a solid crust. From one observer and another has come confirmation, more or less strong, of this new idea. The solar analogy of the rotation, varying with the latitude and being quickest at the equator, had been known as long ago as the time of Cassini, who even hinted at the similarity of Jupiter markings to sun-spots, a similarity more generally recognised since the careful and prolonged observations of Denning and others on the motion and appearance of the spots and belts. The experiment of Bond, often since repeated, renders it only very possible and not by any means certain that Jupiter's surface, or what appears to us as surface, whether of gaseous body or overlying cloud strata, is self-luminous, as it would be if really incandescent. The albedo is determined to be as high as 70 per cent., and since this is near the limit of the whitest surfaces known in nature, and moreover since Jupiter appears not white but tinted, and also marked with dusky spots and bands, it is an easy inference that the 70 per cent. is not all due to albedo.

On the other hand the spectroscope shows little but the ordinary Fraunhofer spectrum. There are other lines present, some due to a damp atmosphere and some unknown, one strong band in the red having been identified by Professor Vogel with a line seen in some red-star spectra. Practically all the admissible spectroscopic evidence is in favour of a cool atmosphere and against an incandescent surface. In addition to this the satellites of Jupiter when crossing between us and the planet have a way of casting dark shadows. They are as a rule brighter than the edges of Jupiter's apparent disc, but not so bright as the centre; thus on occasions one or other of them appears quite black against the planet, so that here the evidence is conflicting. The first satellite only appears a little dusky, not dark; the second has a high albedo and never shows dark at all; the third and fourth appear to be of variable brightness. These last two have been proved by careful observations at Arequipa, and at the Lowell Observatory, to have reached the advanced stage of development attained by the Moon, Mercury and Venus, and to keep always the same face to their primary, a conclusion asserted more than a century ago by Herschel, but long considered doubtful, and especially as the first and second satellites do not show this peculiarity.

The first satellite revolves about Jupiter roughly four times a week, the second twice, the third nearly once, while the fourth takes rather more than a fortnight, so that a very simple relation approximately holds between the several periods. But after the lapse of centuries this satisfactory harmony has been disturbed, first by Barnard's discovery in September 1892 with the 36-inch Lick telescope of a fifth satellite, nearer to the planet than the first satellite, and revolving about twice in twenty-four hours. Since Jupiter's rotation period is nearly ten hours, this means that the fifth satellite is analogous to the outer satellite of Mars, and takes more than two Jovian days between rising and setting. It is quite small, probably not more than 100 miles in diameter, and only visible in very large telescopes under good conditions. The discovery bears eloquent testimony to the acuteness of Professor Barnard's vision, as the satellite is of the thirteenth magnitude, and never moves far from the glaring disc of Jupiter. Still more recently, in December 1904 and January 1905, two additional Jovian satellites were discovered by C. D. Perrine at the Lick Observatory, on photographs taken with the Crossley reflector. Like the fifth satellite they are quite small and faint, but unlike it they are far away from their primary, and take nearly as many of our months to revolve as the fifth satellite does hours—roughly, 600 Jovian days. It was for some time maintained that they were not satellites, but distant members of the asteroid group, whose mean motion would be sufficiently near that of Jupiter to compel them for some time apparently to accompany his system ; but further observations negatived this suggestion, and more attention has been devoted to the question of origin. It has been conjectured that Jupiter captured them from the outermost asteroids, so that they are new members of the system. On the other hand, it has been argued that distant satellites must be the oldest ones ; but as this theory depends on the nebular hypothesis, it can scarcely command unquestioning support. It might be expected that, as the period of these new satellites is about sixteen times that of the old fourth satellite, and that of the fifth satellite about one quarter of that of the first satellite, other members are still awaiting discovery to complete the gaps thus introduced in the simple relations of the system—one between the orbits of the fifth and first, and three between those of the fourth and the two newest ones. There is only the slightest conjectural foundation for this; but the very smallness of these new members renders it possible that they are analogous to the asteroids, and that increased optical power may betray many more such satellites.

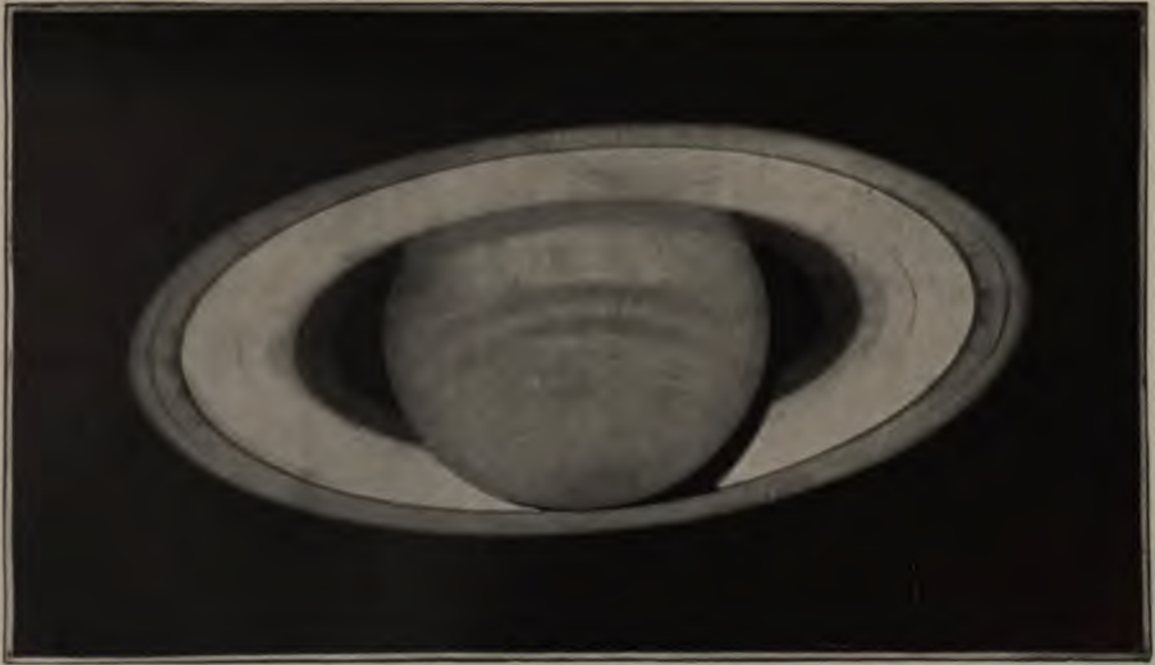
Of greater interest, however, than insignificant specks like these are the conspicuous and variable markings, spots, and bands on the apparent surface of Jupiter, of which the most famous is known as the Great Red Spot, to which attention was first drawn by Niesten of Brussels in 1878. In 1879 it had become much more conspicuous in colour, and in actual section was three or four times as large as the earth, though its elongated shape would not have allowed the earth to pass quite through its circumference. A neighbouring white equatorial spot rotated in nine hours fifty minutes, while the red spot took five and a half minutes longer. Obviously they could not both be fixed, and assiduous observation proved that neither was. The red spot faded away in 1883, but revived again, and in fact may be regarded as a feature neither temporary nor permanent, in the ordinary sense; for on the one hand its persistence is far too great for a cloud, as we understand it, and its disappearances may be only due to the interposition of a veil of some kind; while on the other hand its position, size, colour, and motion are variable. In spite of persistent observation by Denning of Bristol, Stanley Williams of Brighton, and others in this country, and by G. W. Hough of Chicago and others elsewhere, we know very little of the nature of the spot and the causes of its variation. It is not known whether it is higher or lower than the surroundings. A dark spot on the same parallel overtook it in July 1890, but instead of settling the vexed question by passing over or under the red spot, it simply skirted it in a manner reminiscent of Stockton's celebrated little romance, "The Lady or the Tiger." In recent years the red spot as such has been very little seen, though there is evidence that its effect in interfering with neighbouring bands has not disappeared.

Discussion of series of drawings and photographs has led Stanley Williams to differentiate between nine principal currents in different latitudes, but not necessarily quicker near the Jovian equator, so that the solar analogy does not hold completely. Spectroscopic determination of the rotation by Bélopolsky and Deslandres shows that Jupiter exaggerates his size, since the linear velocity shown at his equator is less than that inferred from his apparent size and observed angular rotation. The peculiar markings round the equator, with the appearance of "port-holes," sometimes equipped with "guns," have been found in their variations to conform fairly well to a "sun-spot" period, but we are still far from being able to differentiate between cause and effect in regard to this tantalising periodicity, whose frequent emergence from analyses of totally different phenomena have earned for it the title of "the universal pulse of the solar system" given it by Professor Hahn.

If Jupiter's low density and variable cloud system are evidences of an early stage of development and a high internal temperature, still more is this the case with Saturn. In bulk not far inferior to Jupiter, yet lighter than water, and with density increasing inwards, the outer envelopes must consist of heated gases in active circulation.

Markings on Saturn are inconspicuous as a rule, but bright spots appear from time to time, and from these a rotation period of about 10¼ hours has been deduced for the equatorial portions. In 1903, Barnard with the 40-inch Yerkes refractor, announced a bright spot in a higher latitude, and from numerous observations by many observers a period emerged of about 10 hours 38 minutes. The state of affairs on Saturn's surface thus appears to be "like Jupiter, only more so"; for the difference of these periods points to an equatorial current on Saturn far stronger than anything yet deduced for Jupiter.

But the conspicuous feature of the system is the ring, whose inconsistent appearance startled Galileo until explained by improved optical power. Edgeways hardly to be seen at all, it was nevertheless considered, until the middle of the nineteenth century, as a solid body, or rather as more than one, for the



SATURN

1956 :

most conspicuous division in the ring system was noted more than two centuries ago by Cassini. But immediately after that time, first G. P. Bond, from their appearance, and then Benjamin Peirce, on theoretical grounds, began to deny the solidity, which had been such a stumbling-block to Laplace's theory, inasmuch as by it stability was hardly possible. In 1857 Clerk Maxwell, for the Adams Prize at Cambridge University, proved that the rings were not even fluid, but that they must consist of an aggregation of fine particles revolving independently, with periods determined by their distances from the planet. Thus an idea put forward as a speculation in the seventeenth century, and afterwards in the eighteenth by J. Cassini and Thomas Wright, was mathematically demonstrated as the only possible solution.

Confirmation along other lines has not been wanting. Evanescent markings or divisions testify to the unequal rotation, and slight irregularities in the shape of the rings and of the shadow of Saturn on them confirm variety of orbital planes. The unvarying brightness of the outer rings under different angles of illumination is additional proof of the correctness of Clerk Maxwell's deduction. It has been urged against this photometric evidence of Professor Seeliger's that it does not account for the dusky inner ring. Seeliger's reply is that the inner ring is composed of similar particles not so thickly strewn, and that the dusky appearance is due to continuously recurring shadows. The inner dusky ring is transparent, for Barnard, in 1889, saw Iapetus, one of Saturn's great satellites, in the shadow of the ring, showing that sunlight could pass through.

An even more delicate confirmation is due to Professor Keeler, who, in 1895, at the Allegheny Observatory, proved by spectroscopic observations of rotation that the outer part of the ring travelled more slowly than the inner, the exact opposite of what would happen if the rings were solid. The innermost portion, in fact, revolves about twice as fast as Saturn itself.

From comparison of old drawings made by Huyghens with more recent ones, Otto Struve, in 1851, suspected that the rings were shrinking towards Saturn, or that, at any rate, the space between Saturn and the innermost ring was diminishing steadily. He therefore made very careful measures, which he was able to repeat in 1882 when Saturn had performed a complete revolution round the sun and returned to similar conditions. It appeared then that Huyghens' drawings had been too uncertain, the observed change being rather in the nature of a slight spreading out of the rings both ways, and even that very uncertain, though on Clerk Maxwell's theory such a change was likely. Subsequent measures, even with the Lick telescope, show no change. The various gaps in the continuity of the ring system, of which the Cassini division is the chief, indicate distances at which no particles revolve, and these have been connected by Kirkwood's law with the periods of Saturn's "ordinary" satellites, just as the gaps in the asteroid zone were referred to that of Jupiter. Kirkwood himself showed that the Cassini division represented a period nearly commensurable with those of four of Saturn's moons, then considered to be eight in number (in addition to the countless constituents of the rings, which are, strictly speaking, so many separate satellites).

The eighth of these moons, counting outwards from the planet, is Iapetus, whose peculiarity is a great variability in brightness, the variation being in the ratio of 2 to 9, and the satellite being nearly invisible east of the planet while conspicuous west of it. The inference first drawn by Herschel, and since photometrically confirmed by W. H. Pickering, is that Iapetus always keeps the same face towards Saturn (which we have seen in other instances to be a normal state of affairs with "old" satellites), and that its surface is half bright and half almost dark. As regards the number of recognised Saturnian moons, in 1899 W. H. Pickering announced that on plates taken at Arequipa in 1898 another satellite of very small magnitude was shown. For lack of confirmation this was long doubted, but unexpectedly great eccentricity in the orbit of the new satellite was partly accountable for this, in addition to the fact that the discoverer's energies were just then devoted to his great work on the moon. In 1904 search on Arequipa plates, pushed rather further from Saturn's limb, revealed the presence of another object, in all probability the same as that shown in 1898; and Saturn's ninth satellite was soon officially recognised and the name Phœbe assigned. Its distance from Saturn is so great, about eight millions of miles, that it takes a year and a half of our time, or more than 1000 Saturnian days, to revolve about its primary, and its orbital eccentricity, nearly ¼, is almost twice as great as the greatest previously known for a satellite. Another unique feature of the motion of Phœbe is that it revolves about Saturn in the opposite direction to that usual in the solar system. As was afterwards suggested in the case of Jupiter's new satellites, the question arose as to whether this satellite was captured by Saturn, but the discoverer maintained that its retrograde motion was originally common to the system, but that after the birth of Phœbe solar tides pulled the planet over. It is unfortunate for this theory that Jupiter's distant satellites do not confirm it.

F. J. M. Stratton was recently awarded the Smith's Prize at Cambridge University for a laborious mathematical analysis of the possibilities of planetary inversion as exemplified in the motion of Phœbe. His conclusions, though necessarily starting from data in part conjectural, point to the possibility of Pickering's suggestion being well-founded, and of solar tides pulling Saturn over from an obliquity greater than a right angle (or retrograde motion) to one less than a right angle (direct motion) after the birth of Phœbe, which might preserve its original direction of revolution. In view of the fact that the satellites of Uranus and Neptune also revolve in the retrograde direction, he concludes further that the tidal effect of satellites themselves is to prevent the obliquity reaching a right angle either way, so that Saturn's obliquity, having once passed that critical value, has been steadily diminished by such an action due to the great satellites and the ring, while Uranus and Neptune, being less subject to solar tidal action, have been actually prevented from turning over in the manner claimed for Saturn, by the relatively more important tides of their own satellites. We must, however, postpone further consideration of questions of evolution to the end of the chapter.

The discoverer of Phœbe has since announced a tenth satellite, possibly forming with the small seventh satellite, Hyperion, part of a zone of asteroidal satellites, similar to that suggested by the two latest members of Jupiter's system.

Saturn's spectrum appears similar to Jupiter's, showing the same "red-star" line, and doubtful traces of aqueous vapour. The ring certainly has no atmosphere.

The advance of knowledge in regard to Uranus is very slow. It appears to conform to the fashion in major planets of rotating in about ten hours, but the few markings visible on its disc in terrestrial telescopes are of an uncertain character. Its satellites revolve in a plane almost at right angles to the ecliptic as if turned half-way over, but it seems on the whole likely, though different determinations are discordant, that the planet itself rotates in a plane quite different from that of the satellites, which, moreover, revolve like Phœbe in a retrograde direction. It appears distinctly flattened at the poles, and Barnard, on the assumption that the observed bulge was equatorial, deduced an angle of

28° between the two planes in question. Its diameter is less than half that of Saturn, and Dr See calculates the compression as $\frac{1}{25}$. Spectroscopists have differed in their conclusions in regard to Uranus. Huggins obtained a simple Fraunhofer spectrum indicating reflected sunlight, but others have found quite different conditions, and although a fluted spectrum asserted by one observer was proved to be an illusory contrast effect by Professor Keeler, there still remains strong testimony as to the appearance of six broad bands instead of a Fraunhofer spectrum, one band being F of hydrogen, hence presumed to exist free in the Uranian atmosphere, and another the same stellar line in the "red" shown by Jupiter and Saturn. The others are not so clearly identified, though Keeler attributed a broad band in the yellow to water-vapour as shown in the earth's atmosphere.

Neptune, still further off, and of about the same size as Uranus, presents hardly any features. Maxwell Hall in Jamaica deduced a rotation period of eight hours from some temporary fluctuations of brightness at the end of 1883 and again a year later, but no one else seems to have confirmed the result. Neptune is, however, provided with a satellite, whose plane of motion shifts rapidly, owing, it is assumed, to the effect of the equatorial bulging of Neptune. Tisserand and Newcomb have arrived independently at a result giving a limit of $\frac{1}{85}$ for the compression, from which it was inferred that the rotation of Neptune must be slower than that of the other major planets. The inferred direction of Neptune's equatorial plane was corroborated by Dr See's announcement of very faint bands seen in the Washington equatorial, but Barnard could not see anything of the kind with either of the great telescopes of Lick or Yerkes. See has also calculated a compression of $\frac{1}{85}$ and a period of rotation of nearly thirteen hours from a discussion of more recent data.

Systematic photographic measures of Neptune and its satellite have for some time been regularly taken at Greenwich, and these promise in a few years' time to resolve definitely some of the uncertainties of the system. In order to photograph the satellite a length of exposure is required that would be far too long for Neptune, whose image would spread so that the photograph could not be measured. But by means of an occulting shutter, which cuts off the light of Neptune from the plate, the planet is given only a short exposure, and the satellite a much longer one, and the resulting images can be measured with great accuracy.

From the fact that some periodic comets go out as far as Jupiter's orbit, and others as far as Neptune's, while yet others go further still, it has been conjectured that these last indicate the existence of one or more planets beyond Neptune. Professor Forbes of Edinburgh has computed elements for one such possible planet with a period of about 1000 years, and suggested another with a period five times as great. Moreover, Professor Todd, from the residual errors of Uranus after the effect of Neptune was eliminated, computed by

Adams' method a position for an exterior disturbing body. His result gave a direction closely agreeing with that of one of Forbes' conjectured planets, but with a period of 375 years instead of 1000. So far, however, the most diligent photographic search has failed to reveal any such object, and in any case the analogy of the newest discoveries in distant satellites would suggest that the outermost members of a system are probably disproportionately small, and that even the very slight motion apparent in such distant objects would tend to diminish the probability of a speck on the limits of photographic visibility from registering its position on the plate, while at the same time there is no hope of being able to allow for that motion from any theoretical conjecture. An attempt has been made to infer the position and elements of such a body from residual perturbations of Halley's comet, the approaching return of which lends encouragement to the investigation.

# CHAPTER XXVI

## THE SOLAR SYSTEM

IT may be as well at this point to refer once more to the subject of evolution touched upon in an earlier chapter, and to indicate briefly some of the lines of conjecture whose consideration would have seemed premature before dealing with the planets themselves. The researches of Professor Darwin in particular, on tides and tidal effects, supply possible explanations for many of the apparent anomalies of the system. On the hypothesis of Helmholtz, now generally admitted, that the sun's heat is maintained by very slow shrinkage in volume, reasoning backwards through long æons suggests a time when the sun was enormously more vast in extent, so that even apart from the nebular hypothesis the effect of solar tides was conceivably far greater than it is now. But the direct effect of tidal friction on a rotating satellite is proved to be to retard the rotation until its period becomes equal to that of revolution. This effect, familiar in the case of our own moon and recognised in several other instances, supplies a reason for some of the diversities of the planetary systems. Laplace's theory of portions of rotating nebula successively breaking off from the main body, owing to the too great velocity required by gradual condensation, of necessity involves the conclusion that the portions breaking off would at once rotate in the opposite direction. The effect of tidal friction, as pointed out by Kirkwood, would however, in general, speedily bring them to the state of always turning the same face to their primary, thus making the motion of rotation direct though slow, so that when the falling off of the tidal effect through shrinkage of the parent body permitted the rotation velocity to increase, the motion would remain direct. It is uncertain whether this completely satisfies the conditions of the most distant planets, as it is maintained by some that the primitive system went no further than Saturn, and that Uranus and Neptune joined later, but the fact that their satellites do still move in the retrograde direction does not demand such an assumption, as it is almost a corollary to Kirkwood's reasoning that the diminishing effect of solar tides would be more marked in those early stages and might have been just too small to overcome the retrograde motion of the outermost bodies, including, as has been hinted before, the furthest satellite of Saturn. Another effect of the diminution of velocity by tidal friction is gradually to increase the distance between planet and satellite in accordance with Kepler's third law, so that the ultimate distance of a satellite depends on its tide-raising capacity. This is given by Nolan as a reason why Jupiter's small fifth satellite is so close to the surface of the planet, namely that it is too small to cause an appreciable tide.

The two innermost planets moreover have no satellites, and a very similar reasoning shows that their rotation was so speedily brought to agree with their revolution that no opportunity was given for the throwing off of a satellite. The critical case of the earth, between planets with no satellites and planets with more than one, is quite unique, for our solitary moon is far bigger in proportion than any other satellite in the solar system. The accepted explanation consistent with the tidal theory is that it was so long before the tidal effect diminished sufficiently to allow the earth to throw off a satellite, that its condensation and solidification had reached a far later stage than was the case with the outer planets, so that the moon, when it finally did break away, with a calculated rotation period of less than two and a half hours, was quite a considerable fraction of the whole mass. How long ago even this took place may be roughly inferred from the fact that the lengthening of the month in historical times is almost inappreciable, but that nevertheless, the moon's rotation has slackened since that far-off epoch in the ratio of two or three hundred to unity.

It is also fairly clear that such a change could not have occurred in such a way to any of the other planets, as it is only the comparatively great size of the moon that rendered it capable of producing sufficiently powerful tides. All other satellites in the solar system must have been thrown off, if thrown off they were, before the condensation of their primaries.

One other notable anomaly is the case of Phobos, the only ordinary satellite revolving faster than its primary, a condition of things whose possibility was denied by Laplace. Wolf, following Roche's ideas,[1] considers that this may be due to Phobos having been thrown off from a high Martian latitude, where the velocity would be smaller than in the normal equatorial case. And it seems probable that since the tidal effect in the case of a satellite travelling faster than its primary is in the opposite direction, and tends to shorten both periods, the ultimate fate of Phobos will be to draw nearer and nearer to the surface of Mars, and at last rejoin the parent body.

The interior portion of Saturn's ring also appears to rotate much faster than Saturn, but this cannot be explained in the same way, though, as we have seen, there is some slight evidence that it may be drawing nearer to the planet's surface.

Variations of the nebular hypothesis have from time to time appeared, in the endeavour to reconcile various anomalies by modifications of Laplace's original idea. They mostly, however, succeed in destroying the simplicity which was the great charm of the hypothesis, and are not much more satisfactory in other ways. Roche of Montpellier, principally remembered for his announcement in 1848 of a limiting distance of about two and a half radii from a planet, within which a stable satellite could not exist (known as Roche's limit), was also responsible for the suggestion referred to in the case of

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[1] See below.

Phobos of "trainées elliptiques" coming from the polar regions to disturb the distribution of angular momentum and cause equatorial disruption at intervals supplying a sort of theoretical basis for Bode's Law. Faye in his "Sur l'Origine du Monde," 1884, departed more from tradition than Roche, substituting for Laplace's stratified nebula a vast cluster of particles, and on his hypothesis Uranus and Neptune are the newest planets, the others having been formed earlier and drawn in towards the sun; as its gravitating power increased in consequence of the meteoric condensation that would take place in the cluster. In favour of his scheme it can be said that it was the first that could admit comets, in which Faye was particularly interested, as original members of the system, and also evaded some geological difficulties connected with the assumed age of the sun, by considerably antedating the world's beginning. Against it is, among other objections, the improbability of a limit of similar conditions falling between Saturn and Uranus, and not in the more obvious transition region occupied by the minor planets which separate the solid planets from the outer nebulous globes.

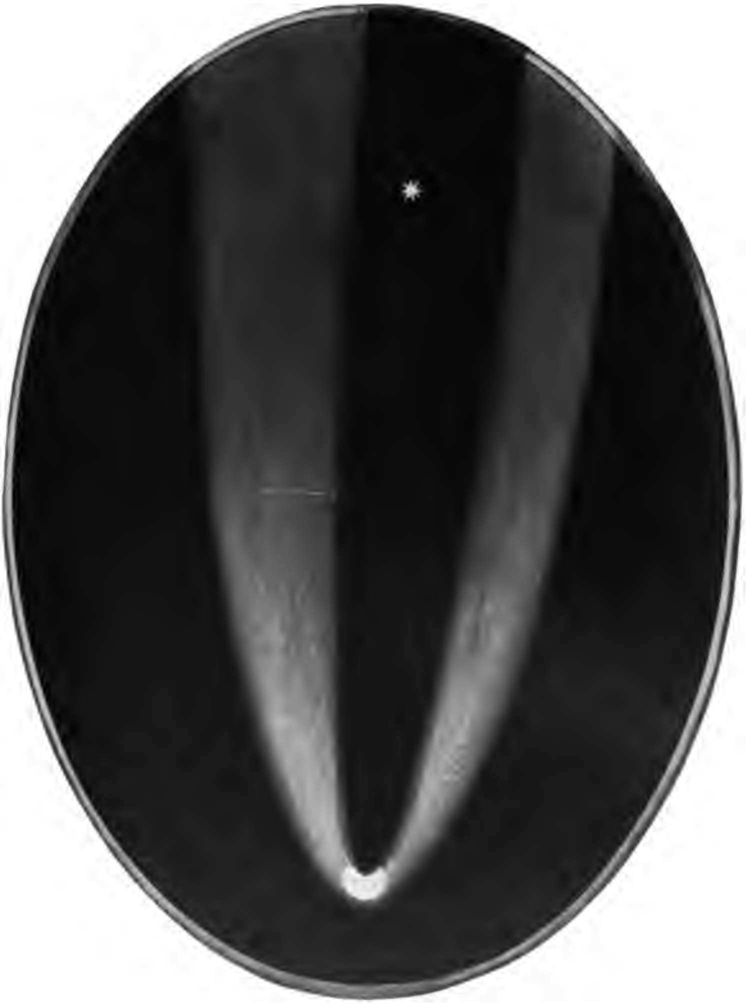
More recently, R. du Ligondès evolved in 1897 a modification of Faye's doctrine, attempting to account mechanically for the phases of condensation required. Haphazard motion of particles in different directions, tending to bring about collisions which by destroying angular momentum would cause a continual fall of suddenly arrested matter to the centre of the system, was the feature of the scheme by which he was led, as a corollary, to the statement that the original state of things is best illustrated by casual unperturbed comets. The original nebula of Laplace was gaseous, the more recently suggested ones more like clouds of dust, but beyond the idea of primitive nebula of one kind or the other little can be said to survive modern criticism. We have already referred to the planetesimal hypothesis as likely to hold the field in the near future, and shall return to the subject when we reach the discussion of nebulæ. It may be stated here in connection with anomalies in the solar system, that Professor F. R. Moulton claims by the new hypothesis to account for nearly all the observed general features of the system, such as the equatorial acceleration of the sun, the infrequency of retrograde motions, the small eccentricity, high temperature, low density and rapid rotation of the larger planets, the rapid motion of Phobos, the eccentricity of Phœbe, and the various anomalies of asteroid orbits. Admitting this, it seems to account for all the facts explained by older hypotheses, and for at least some where they have failed, and to have so far met no insuperable obstacle. It is supported on geological grounds by Professor Chamberlin, who sees in it a plausible explanation of such refractory problems as the distribution of land and water and the carboniferous era.

# CHAPTER XXVII

## COMETS, METEORS, ZODIACAL LIGHT

THE remaining members of the solar system must not be overlooked. Comets, which even now to the disordered imagination (or rather perhaps to the business instinct) of a Zadkiel, are regarded as portents, lost their terrifying character to a considerable extent when the recognition of their periodicity in some cases brought the inference that their motions were subject to the same laws as those of the planets, the chief outstanding difference being the eccentricity, which, though in occasional instances not much greater than those of one or two exceptional asteroids, approaches, reaches, and possibly exceeds unity, thus providing highly elongated ellipses, parabolas, and, it may be hyperbolas, for the apparent orbits. In the first of these cases the comet is periodic, but most observed comets belong to the second class, or rather cannot be distinguished from it owing to the extreme length and uncertainty of the periods. The third case, of which there are few absolutely certain instances strictly speaking, is that of what may be called temporary members of the system, though a really parabolic orbit may also be regarded as that of a temporary member also.

The first great comet of the period we are now considering was the celebrated 1858 comet of Donati, the finest comet within living memory, though not considered quite so bright as that of 1811. Visible to the naked eye for sixteen weeks, and to the telescopes of the time for nine months, this comet was exhaustively observed. Its brightness, which at maximum surpassed that of Arcturus, was a notable instance of the great increase in actual brightness near the sun, which far exceeded that predicted by theory from its relative distances from the earth and the sun. Its tail, or rather tails, were well placed for observation, and as one after another became visible, the main portion curved, with two straight tangents, the idea suggested by Olbers, that these tails were different kinds of matter unequally repelled by the sun, seemed almost to require no further proof. The head of the comet was also in violent agitation, throwing off veils of gauzy matter towards the sun to curve back like fountains after a comparatively short time, the general appearance being that of a hollow cone or cones seen brightest at two opposite edges. This comet was too early for the spectroscope; its brightness was analysed only by the polariscope, an instrument often disappointing in its results. In this case the analysis showed no evidence that the brightness, although only about 3 per cent. of it could be accounted for by its position, was at all intrinsic, such as might be caused by a great increase of temperature in careering with increasing velocity through a medium even of great tenuity, or possibly by the friction of



DONATI'S COMET, 1858. (GREENWICH)

C 199.07

its agitated parts, or, as was commonly suggested, by ignition in the heat of the sun's rays. Simple reflected light was the unsatisfactory verdict of the polariscope, and there was then no higher court of appeal. So all that issued from the mass of observations was a very accurate determination of the orbit, giving a period of 2000 years.

In 1861 the earth passed for some hours through the tail of a comet, to the depth, computed by Liais from the observations of the discoverer, John Tebbutt, of New South Wales, and his own at Rio de Janeiro, of 300,000 miles. There was no other evidence except the observed motion of the comet that the earth had actually passed through the tail, which caused no apparent effect, electrical or otherwise, distinctly traceable to the unusual environment. Within the next few years two celebrated comets were discovered, first designated simply comets 1862 III. and 1866 I., but since the labours of successive investigators were brought to a final conclusion by Schiaparelli, known as the comets of the Perseids and Leonids respectively. The first of these reached practically the second magnitude, while the other was very faint and showed no tail. The connection between comets and meteors definitely pointed out by Schiaparelli, had been in a hazy sort of way present to the minds of investigators for some time before, though the original idea of meteors was that they were akin to firedamp, small clouds of marsh gas suddenly ignited in the air. Chladni, in 1794, stated in concrete form a notion vaguely entertained by Halley that they were cosmic atoms, made white hot by friction in the atmosphere. Velocities were determined from simultaneous observations sufficiently far apart to indicate the height of the meteor, and these velocities turned out to be great enough for bodies moving like planets. Laplace and others, however, maintained that they were ejected from lunar volcanoes. But the uncertainty was to a great extent dispelled by the marvellous shower of meteors on the night of November 12, 1833. The display was at its best in America, and at Boston it was calculated, actual counting being impossible, that 240,000 were visible during the nine hours from the time the shower commenced until daybreak put an end to the sight. The illuminating feature of this splendid apparition was the circumstance that all the meteors seemed to come from the same part of the sky, the sickle in the constellation of Leo, all paths accurately drawn diverging from a single point, which had no relation to the earth, but moved with the stars. It was speedily deduced that the meteors were describing orbits round the sun, and since a display on a small scale had been seen just a year previously in the old world, these orbits must meet that of the earth at the point reached by it on November 12.

Meteors were at once recognised as providing a new field of astronomy. Humboldt had witnessed a shower almost as brilliant on the same night of the year 1799, and had also noted that the bright paths were directed from a single "radiant," but no theory had been based upon his observation; and the first guess at the "Leonid" period, by Professor Olmsted of Yale, was a half-yearly one, on the assumption that the aphelion was reached on November 12 every year. But the display in 1834 was not a great one and each year saw still less, and the next conjecture, by Olbers in 1837, was founded on Humboldt's observation and suggested a period of thirty-four years, fixing the next great display in November 1867. The August meteors or Perseids were also the subject of inquiry, and the fact that they also appeared annually discredited Olmsted's idea that the Leonids were a cloud of particles with a short period, suggesting instead a ring of particles extending round an orbit nearly, if not quite, intersecting that of the earth at the point reached by it on November 12.

Nothing more was done in the way of advance for a long time, in fact, until Professor H. A. Newton took up the investigation at Yale in 1864. Consulting old records, he identified the Leonids with a historic display on October 13, 902, when Taormina was captured by the Saracens, and deduced a period of 33¼ years from successive appearances which recurred with a retardation of one day in seventy years. He predicted a fine display for the night of November 13, 1866.

Four other periods were found, which would account for the interval of thirty-three or thirty-four years, but the loss of one day in seventy years provided Professor Adams with a method of mathematical investigation, since it was due to the advance of the node of the orbit caused by planetary perturbations. His result, which dismissed as impossible all but the 33¼ year period, was not reached until a few months after Professor Newton's prophecy was fulfilled.

Preparations had been made in advance. At Greenwich, for instance, the new branch of routine observation was introduced in 1865 and a fair number of Leonids observed. But the great display came on the predicted night in 1866, bringing crowds of meteors, many very bright, and some as bright as Venus. The greatest rate estimated for an hour averaged more than one per second, the actual numbers recorded at Greenwich being 4858 between one and two o'clock on the morning of the 14th.

Schiaparelli's investigation, stimulated by this magnificent display, showed that meteors travel much faster than the earth, so that their orbits are much larger than that of the earth, and their motion like those of comets rather than of planets. He next inferred that, in general, comets and meteors come from outside the solar system, are temporarily drawn into it by the sun, and occasionally kept within it by the retarding action of a planet. Lastly, he identified the orbit of the Perseids with that of the bright comet of 1862. Soon afterwards, Dr Peters, of Altona, from Le Verrier's new elements of the Leonid swarm, identified the orbit with that of the comet of 1866, Schiaparelli immediately afterwards independently confirming the result. Professor Weiss similarly connected the April Lyrids with a comet seen in 1861, and the Andromedes of late November with Biela's comet. Professor

Alexander Herschel took up the subject with enthusiasm, and by 1878 compiled a list of no fewer than 76 comets, known or suspected to be connected with meteor swarms.

The underlying principle of this idea of identity or close similarity between comets and meteors had made its appearance more than once before, and, in 1861, Professor Kirkwood had argued, from the known appearance of Biela's comet in two portions, that the sun had an effect on comet nuclei, whose tendency was to split them up, and suggested that the periodic meteors might be the débris of old shattered comets, whose cohesion being destroyed leaves them nothing but a range of small particles distributed along or near the old orbit.

From the fact that the main swarm of the Leonids seemed to take about three years in passing the point of approach to the earth's orbit, while the Perseids seemed more evenly distributed, Le Verrier argued that these are successive stages of disintegration, the swarm lengthening out until it reaches the stage of a closed ring of particles. He inferred not only that the Perseids were an older formation, but assigned a date for the first appearance of the Leonids in the solar system in A.D. 126, when Uranus, by his calculations, must have been close to the parent comet of the whole system, the comet of 1866 being only regarded as a fragment.

But Biela's comet, which appeared in two portions in 1852, had not been seen since, though carefully sought, and it was naturally suggested that perhaps this might be regarded as a crucial test. Previous showers from the point near $\gamma$ Andromedæ, where the orbit of Biela's comet appeared to meet the earth's track, had been noticed on December 6, 1798, and again in 1830, 1838, and 1847. So Weiss (as we have seen), and, independently, d'Arrest and Galle, inferred that the orbits were identical, and that the date would be getting earlier in the year, since, owing to the motion of the comet being retrograde, the motion of the node would be in the opposite direction to that of the Leonids. The Andromedes were duly observed on November 30, 1867. The comet was due in 1872, and Galle suggested November 28 as the probable date of the swarm of meteors into which the comet might have broken up. It actually arrived on November 27, when the display was nearly as striking as that of the Leonids six years before, the Andromedes or Bielids being slower in apparent motion, as they were travelling to overtake the earth instead of meeting it. At times they were said to average four or five per second, and some were fire-balls apparently as large as the moon. A search suggested by Professor Klinkerfues to see if the comet itself had gone by during the swarm resulted in the discovery of a faint comet, which, though not Biela's, probably belonged to the system. The period of the comet being about 6½ years, a return of the swarm was expected in November 1885, and expectation was more than fulfilled, for the swarm, where not masked by cloud, showed more numerous and larger meteors than that of 1872. Professor

Newton estimated 75,000 per hour in the densest part of the swarm, and a density of one to a cubical space of 20 miles edge. A near approach to Jupiter in 1841 was the probable deciding factor in the disintegration of this system. In 1892 a fine shower of Andromedes was seen in America, though far less striking than that of 1885. Its appearance so early as November 23 suggested that this was not the main swarm, but an associated branch. The next expected displays have failed to appear in any numbers—the first in 1899 and the next in 1905—so, perhaps, planetary perturbations have so modified the orbit that it no longer passes near enough to the earth to display more than a few stragglers. The Leonid display expected in 1899 also failed, as had been predicted by Dr Johnstone Stoney, of Dublin, and Dr Downing, of the Nautical Almanac Office, would be the result of perturbations by Jupiter and Saturn. Search at the usual dates will be maintained, as meteors are more frequent at those special epochs; but there is now no definite expectation of any such vivid displays as the various November showers of 1833, 1866, 1872, and 1885.

Progress has been made in the direction of accuracy in the determination of paths by the use of a "meteorograph," a camera directed near the expected radiant, so that any meteor bright enough to impress its trail on the plate could be accurately referred to the stellar images surrounding it, comparison with a similar photograph of the same region from a different station yielding exact data for the determination of the real path, height, and velocity of the meteor.

From the connection, now regarded as proved, between comets and meteors, emerges the certainty that comets are temporary bodies, that they break up into smaller comets or streams of meteors, and ultimately to some extent are absorbed by more permanent members of the solar system. The question whether aerolites, meteoric stones that actually fall on the earth without being pulverised, are of the same class, is not so well determined. Tschermak and others postulate a different origin. Some, following Laplace, assume a lunar volcanic source, others conjecture a source in one or other of the great planets; others again, like Sir Robert Ball, attribute them to terrestrial volcanoes of a bygone day, whose missiles, hurled out when the explosive force was far greater than it is now, have been constrained to revolve about the sun in orbits bound at intervals to pass through the point of origin, and so, in course of time, to fall to earth again. It has even been supposed that they may come from the sun. But no real distinction can be drawn between the meteors that are seen to burn up and those that burst into sizable fragments, or reach the earth without attaining an explosive temperature. Detonating fireballs have been seen apparently belonging to a known radiant, and aerolites have occasionally fallen during meteoric displays, the coincidence being very likely fortuitous. Professor Newton considered that the larger meteorites, whose paths were traced by him, appeared to have their perihelia in the outer portion of the space between the earth and the sun, to correspond more closely with short-period comets, and to be more planetary in character than the recognised comet-meteor swarms. The pursuit of meteoric astronomy in this country has brought to the fore an amateur observer, W. F. Denning of Bristol, to whom reference has already been made in connection with planetary observations. By unremitting diligence in watching for known showers, and in collating the results of other observers, he has computed radiant points by the hundred, and reached a leading place among the world's astronomical specialists, at a sacrifice of his own time and means which has lately found recognition in the form of a Civil List pension. One of his most striking achievements has been the proof of the existence, long denied as impossible, of a distinct class of meteors, whose radiants remained fixed for months instead of showing the daily motion of the earth by a gradual shift in the sky. The first hypothesis was that this could only be explained by the assumption that the earth's velocity was negligible in comparison with that of the meteors, but, as Ranyard calculated, this implies a velocity for them of at least 880 miles a second, which is out of the question. Hence for a long time Denning's discovery was scouted, but the accumulation of evidence still went on, and the fact obtained grudging recognition, while the suggested explanation had to give way, the motion of the meteors, much slower when overtaking the earth than when meeting it, proving that no such enormous velocities were necessary. The contention of Bredikhine that the community of radiant is only apparent, and that the successive meteors belong to several different swarms, seems to postulate such a vast number of coincidences, as to create an equally refractory difficulty. Professor Turner's explanation is at least plausible, in so far as it gets rid of the fortuitous nature of the coincidences; he attributes the successive swarms from the same apparent, but really slightly different radiant, to be due to the cumulative effect of the earth's attraction, exercised at regular intervals, on neighbouring members of the swarm. The details of the explanation are not sufficiently convincing to be regarded as settling the question, which still awaits solution. The Perseid radiant has been questioned for the opposite reason, the motion in a period of nearly six weeks for which separate radiants have been computed being palpably greater than that of the earth in its orbit, but Dr Kleiber, making due allowance for the earth's motion and for its attraction, proved that all the radiants belong to a compact group, of which the comet-radiant is the centre. Even so, however, the Perseid stream is of enormous width.

We must now resume the consideration of comets. The application of the spectroscope was not much too late for Donati's great comet. He himself was the first to obtain a practical result with the new weapon of research, for applying it to a comet which appeared in 1864 and reached the second magnitude, he found the spectrum to consist of three bright bands—yellow, green, and blue, with dark intervals between. This promptly negatived the conclusion suggested by the polariscope observations, and proved the existence of luminous gas. In 1868, observing Winnecke's comet, Huggins identified the bands as belonging to a hydro-carbon spectrum, a similar one being produced by electric discharge through a "vacuum tube" containing olefiant gas. Vogel and Hasselberg increased the resemblance in later experiments by adding a little carbonic oxide. Eighteen comets, from 1868 to 1880, showed the same typical spectrum, but it was impossible to imitate it by any other method but electric discharge, as a continuous current showed only the spectrum of the carbonic oxide. But two comets before that period and some since failed to show the hydro-carbon spectrum. Those observed by Huggins in 1866 and 1867 showed a continuous spectrum crossed by one green ray, generally associated with nebulæ. Comet Holmes (1892) gave a weak continuous spectrum, but the great southern comet of 1901, which also showed an almost continuous spectrum, stifled the theory that the absence of self-luminosity pointed to a state of decay.

In 1874 a much brighter comet, discovered at Marseilles by Coggia, not only enabled Vogel and Huggins, with the addition of Bredikhine, to recognise the typical hydro-carbon spectrum, but also gave Father Secchi at Rome the opportunity of detecting two more bands, in the red and violet. It is probable that these should be visible in the other comets also, but for the absorption of the fainter bands by the atmosphere.

Zöllner's investigations have caused it to be accepted as a fact that the incandescence of the comet gases is not due to heat but to electricity, the effect of the solar radiation and other changes due to the rapid motion of the nucleus being manifested in this way rather than by a simple increase of temperature. It was Zöllner, also, who first definitely formulated the physical theory of repulsion to account for the appearance of comets' tails, though, as we have seen, Olbers had suggested the idea of repulsion. Bessel applied it in the discussion of Halley's comet, and Norton in that of Donati.

Since gravitational pull depends on the mass of a particle and electrical repulsion on its sectional area, it follows that, in dealing with very small particles below a certain limiting size, the repulsion increases relatively to the attraction as the size of the particles diminishes, so that, beyond another limit, different for particles of different substances, the repulsion actually overbalances the attraction. Zöllner pointed out that while the more massive nucleus of a comet obeys the laws of gravitation, the very finely divided particles shed from it in an electrified condition follow perforce the lines of electrical repulsion, and stretch away from the sun in the form of tails.

Bredikhine, whose enthusiasm was kindled by his observation of Coggia's comet in 1874, devoted himself to the subject of this repulsive force, which he computed to be different from what he had computed for other comets a dozen years previously, and a few years later, having completed his investigations of thirty-six well-observed comets, he confirmed an idea, at which he hinted in 1877, by announcing that all comets' tails are divided into three classes, according as the repulsive force is much greater than, nearly equal to, or distinctly less than, the solar gravitation. The actual figures given were—for the first class, fourteen times solar gravity, giving rise to very long, straight tails; for the second, the scimitar tails, a force varying from a half to more than twice the solar gravity from one edge to the other, and having at the axis a value only 10 per cent. in excess of the opposed attraction; and for the third, the short brush tails, from 10 to 30 per cent. of the sun's gravity. The third class does not appear to occur alone in bright comets, in fact, it seems that in general more than one type is present. Brédikhine was, however, not satisfied, as his predecessors had been, to assume that the classes indicated different kinds of matter, but proceeded to identify them from the relations of the requisite forces which he assumed to be inversely proportional to the atomic weights. The three substances he announced were hydrogen, hydro-carbons, and iron. Of these, iron appears often in meteorites, and its presence in comets is quite possible, and the very width of the brush-like tails of the third class shows that iron could not be the only substance represented, a suggestion that would have been far from plausible. Hydro-carbons had been proved to exist in comets, and so only the hydrogen remained doubtful, as it has never been detected in comets by its characteristic spectral emission lines. The modern ideas of light-pressure or radiation pressure, enunciated originally by Fitzgerald, and afterwards by Arrhenius and others, tend to render Bredikhine's chemical theory unnecessary, the various modes of action of the electricity itself being quite sufficient to account for the undoubted type-variations. But this theory is far from being crystallised into dogma, and is still in the hands of critics and experimenters.

Five notable comets appeared soon after the publication of Bredikhine's hypothesis. The first of them was the great southern comet of 1880, conspicuous to the naked eye for eight days, whose orbit so closely resembled that of the famous comet of 1843 that several theories were started to reconcile the apparent impossibility of identifying them as the same object. One very interesting suggestion of Klinkerfues was that the comet appeared in B.C. 371, next in A.D. 1668, and then in 1843 and 1880, a progressive decrease in velocity at each perihelion passage accounting for the shortening of the period. It happened that neither the comet of 1843 nor that of 1880 were observed over a sufficient arc to fix their period with any certainty. An appeal to Bredikhine's hypothesis showed that the great tail of the 1843 comet belonged to the hydrogen class, while no such tail, but only "hydro-carbon" tails appeared in the new comet. Halley's comet having preserved its type was urged as an objection to the suggestion of type-modification.

The next comet, visible from May 1881 to February 1882, provided another problem, for its elements agreed with those of Bessel's comet of

1807. But the long period of observation asserted a period of not 74 years, but nearly 2500 years, so it was in this case concluded that the two comets were portions of a parent comet, one of which lagged 74 years behind the other in the same orbit. The introduction of dry plates for photography took place in time for them to be employed in taking pictures of this comet, which were the first really successful comet photographs, though partial success had been reached so long previously as the time of Donati's comet.

Of Tebbutt's comet Janssen secured a very fine photograph, showing the head and 2½ degrees of tail, and Dr Henry Draper another, showing four times the extension. The latter also photographed the spectrum, as also did Huggins, the result being to confirm the hydro-carbon identification by additional lines beyond the visible spectrum, and also to confirm the polariscope in regard to the presence of Fraunhofer lines denoting reflected sunlight.

Comet Schæberle, though not so bright as that of Tebbutt, was easily visible with it in the northern sky, a very unusual phenomenon. In the following year, 1882, Comet Wells made its appearance, and though not very conspicuous, as it kept near the sun in direction, showed one feature quite new in comet spectra. On approaching the sun the carbon bands died out, being replaced by the bright D line of sodium, observed first at Dunecht, and confirmed by Vogel at Potsdam. Hasselberg argued from this change of spectrum that the luminosity of the vapours must be electrical, as, if the only effect of the sun were to raise the temperature, the sodium line might certainly become visible on that account, but the hydro-carbon lines would also persist. Other peculiarities were revealed by a successful spectrum photograph secured by Huggins.

The last of the five comets was discovered independently by several observers in September 1882, and watched right up to the limb of the sun at the Cape Observatory. After passing the sun it was easily visible in broad daylight, though still quite near the sun, and was thus visible for three successive days. Its path also was so similar to those of the comets of 1843 and 1880 that it seemed to afford another step in Klinkerfues' suggested history, and the theory of retarded motion by a resisting medium near the sun was freely urged again. But in this case the velocities before and after the perihelion passage were carefully compared, and found to show no trace of such a diminution. It was observed, moreover, to a greater distance from the sun than any previous comet, nearly five hundred million miles, the whole arc of observation covering 340 degrees. Kreutz ultimately deduced a period of about 800 years, not agreeing well with those of the other two comets of possibly common origin. But the difference in the length of the orbit is not an objection to the idea of a single parent body, since each fragment would necessarily suffer variations of constants peculiar to itself, and in elongated orbits of this kind the most likely alteration would be the extent of elongation or major axis. Rejecting as probably erroneous the earliest member assumed by Klinkerfues, whose inclusion is a distinct violation of the historic account of Aristotle, this made four members of the same family, and a fifth was discovered at Cordoba in 1887. One other comet is generally assigned to the same group—the one seen and photographed close to the sun during the Egyptian eclipse of 1882.

But the theory of disruption in this particular group received strong confirmation in the great comet of 1882, for early in October the nucleus began to divide first into two condensations, then into three, and then four. Three months later five nuclei were seen in a row "like pearls on a string" by the late Dr Common at Ealing. Moreover, Professor Barnard in October had seen six or eight distinct cometary masses quite near the comet's head. The spectrum of the comet behaved like that of Comet Wells, but showed in addition to the sodium line six bright iron lines in the yellow and green, thus actually confirming an assumption underlying Bredikhine's hypothesis in regard to his third type. The Dunecht observers, Copeland and Lohse, who made this discovery, also computed the velocity from the displacement of the lines. From the sodium line alone Thollon of Nice confirmed this spectroscopic determination of velocity, which agreed very well with that computed from the actual positions of the comet, a strong testimony in favour of the spectroscope for line-of-sight investigations. As the comet receded from the sun, the sodium lines gave place to those of hydro-carbon, as might have been expected.

Another interesting comet, 1889 V discovered by a very successful searcher, W. R. Brooks of Geneva, N.Y., was seen by Barnard at Lick Observatory to have thrown off four fragments, two very ephemeral, one remaining visible for a month, and the last more than three months; all, however, fading from sight before the parent comet, which has since returned alone in 1896 and 1903.

The year 1892 saw seven comets visible together; of these the brightest was only of the third magnitude, discovered by E. Swift, the photographs of which by Barnard and W. H. Pickering showed a great advance in that branch of astronomy; the accompanying plate is a recent example of Barnard's careful work. Another of the seven was the first comet actually discovered by photography,[1] being found by Professor Barnard to have impressed a faint trail on a photograph of stars in Aquila. Another interesting comet of the year was discovered by a London amateur, Edwin Holmes, and was remarkable for two features, one a series of peculiar physical variations, including an unexpected brightening for two days, when it had already faded considerably, having been discovered after perihelion passage; the other, an eccentricity almost low enough for a minor planet, its orbit being also entirely between those of Mars and Jupiter.

One conclusion of W. H. Pickering from observation of the comet 1892 I (E. Swift) was a solar repulsive force nearly forty times that of

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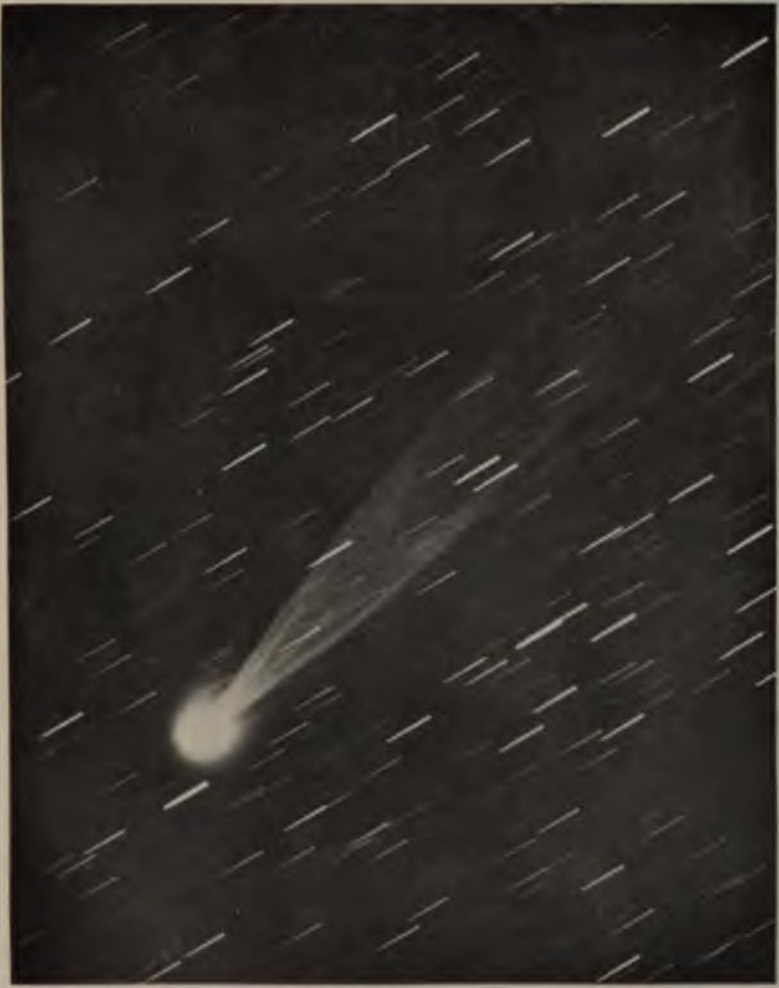
[1] Except, of course, the eclipse comet of 1882, of which no orbit could be calculated.



COMET GIACOBINI, 1905 (BARNARD)

U: y 07

gravitation, or three times as great as that required by Bredikhine's first type, and about double of the limit assigned from theory by Schwartzschild as the maximum possible effect of light pressure. Hussey also found a repulsion nearly as great in the case of another comet, 1893 II.

But the light pressure theory is not the only one, as we have seen. Fessenden in 1896 supposed a negative charge on the sun and on the particles of the tail, and a positive one on the nucleus. J. J. Thomson in 1902 suggested that if the sun's rays induced a comet to give off negative ions these would form a luminous tail. It is not clear how the sun's rays would act, but it is possible that the ultra-violet rays might cause the emission of negative ions which would be repelled by Hertzian waves, if any such were sent out by the sun. Another theory depends on radio-activity in the nucleus itself, different forms of emanation giving rise to different tails.

Among these theories it can hardly be said that one really holds the field. Radio-activity in some form seems the most promising, perhaps because it is so revolutionary in regard to previous physical notions.

Meanwhile an American prize fund awards medals and prizes for cometary discoveries; and within the last five or six years A. F. Lindemann, of Sidmouth, has, by offering premiums for such work, induced various computers to work out the definitive orbits of neglected comets, one of which, 1886 I, is claimed by Svedstrup to be hyperbolic, and to suggest radiation pressure as affecting the observations. C. J. Merfield, of Sydney, an assiduous worker in the same field, had already announced at least two hyperbolic comets, but not so well observed as 1886 I, which was visible for several months.

The brightest comet of the last twenty years was the southern comet of 1901, not seen further north than the Lick Observatory, but very bright in the southern hemisphere.

The next interesting return expected is of Halley's comet, which, according to a recent analysis by Messrs Cowell and Crommelin, of Greenwich Observatory, is due to reach perihelion again by the middle of May 1910, a date agreeing closely with that predicted by Pontécoulant.

Before leaving this chapter we may note that the year 1902 saw not only the largest meteorite for many years in the British Isles, the Crumlin meteorite, over 9 lbs. in weight, but also the largest meteorite known to be in existence, the Mexican meteorite, measuring roughly 13 × 6 × 5 feet, and weighing about fifty tons, consisting mostly of iron, with a fair proportion of nickel, a very little cobalt and phosphorus, and traces of sulphur and silicon.

We must also mention very briefly before leaving the consideration of the solar system, two other almost certainly related phenomena, the "zodiacal light" and the "gegenschein" or zodiacal counterglow. The first is a faint luminosity frequently noticed after sunset or before sunrise under favourable conditions near the equinoxes, especially in low latitudes, often visible all night in the tropics, extending along the direction of the zodiac or ecliptic, broader at the horizon but narrowing towards its visible extremity. It is supposed to indicate the presence of a vast concourse of atoms or corpuscles forming an enormous lens-shaped envelope to the sun, either as distant extension of the outer corona, or possibly impalpable remains of comets and meteors gradually drawing nearer to a final absorption as fuel into the solar fires. One of the numerous theories now discarded as to the source of the maintenance of solar heat was on these lines suggested by J. R. Mayer. But whatever the nature of the zodiacal light, the bodies, if such they be, offer no measurable resistance to the passage even of a comet, and an atmospheric explanation would seem more plausible could one be found to account for the invariable plane of the phenomenon, which is the main argument for its being a solar, or at any rate non-terrestrial, appendage. The gegenschein also belongs to the ecliptic, and is a very faint luminous patch (about twelve degrees by nine in extent), sometimes seen in the direction exactly opposite to the sun. It is generally taken to represent some effect of the earth's cone of shadow, or of the sunlight just beyond the shadow, but the details are uncertain. It has been considered probable, if not certain, of late years that the zodiacal light and the gegenschein are not distinct phenomena inasmuch as they have at times been seen at favourable stations such as Arequipa, connected by faint zodiacal bands, leading to the inference of an extension of the formation, whatever it may be, far beyond the earth's orbit. It has been argued in support of the zodiacal light being distinctly a solar appendage that it is only approximately an ecliptic phenomenon. Newcomb, in 1905, observed it due north at midnight from an Alpine summit, and previous observations had connected it rather with the sun's equatorial plane than with the ecliptic. It has, however, been photographed at Heidelberg and at Flagstaff, and the question of its axial plane ought to be settled in that way before long. If it is clearly zodiacal it may probably represent by-products, waste material left behind in the evolution of the system from the original nebula; if it can be referred to the sun's equator it is not impossible that the solar repulsive force can act at a distance greater than 100,000,000 miles. In any case the light is pronounced on spectroscopic evidence to be reflected sunlight, witnessing the presence of particles of some sort, and we must await the result of further investigations to determine its actual position, distance and extent.

# CHAPTER XXVIII

## THE STARS—CATALOGUES—PROPER MOTION—PARALLAX—MAGNITUDE

TURNING from the vast solar system to the incomparably greater stellar universe, of which it forms but a single speck, though from our point of view a by no means insignificant speck, we must first regard the stars as a whole, supplying as they do the relatively unchanging background against which the motions of the members of the solar system are thrown into prominence.

The importance of accurate standard places of stars was long ago recognised, but comparatively few observatories, at any rate before the middle of the nineteenth century, devoted any considerable part of their energies to the production of catalogues. We have seen how Bessel's appreciation of the monumental labours of Bradley at Greenwich rendered his results available in the "Fundamenta Astronomiæ," and how Lacaille's work in the southern hemisphere provided a supplementary catalogue at the same epoch of the part of the sky not visible at Greenwich. The great work of Bradley has been re-reduced by Dr Auwers of Berlin, with improved constants and tables, and still remains a standard catalogue after the lapse of a century and a half since the epoch of the observations. Another catalogue of nearly the same epoch was that of Tobias Mayer of Göttingen, which has also been re-reduced. With various epochs about the beginning of the last century we find three other well-known catalogues, the Piazzi catalogue from Palermo observations by the discoverer of Ceres, that of Lalande from observations given in the "Histoire Céleste," taken at Paris by various observers and students, and a catalogue of circumpolar stars observed by Stephen Groombridge at Blackheath. The re-reduction of Piazzi's catalogue was recently undertaken, and also the reobservation of all the stars in it at observatories in Italy and America.

Lalande's zone-catalogue purports to contain nearly fifty thousand stars, but as each observation was separately reduced, the real number of stars is not so great, since many of the bright stars have at least five entries, and Vega as many as thirteen. The British Association published the Lalande catalogue with epoch 1800, omitting certain zones near the north pole, which were separately reduced by Fedorenko to the epoch 1790. The moving spirit of the British edition was Francis Baily, who devoted many years to the labour of making other people's observations available, beginning with those of Flamsteed, a century before Lalande. The Paris Observatory undertook the re-observation of Lalande stars, and the results of observations from 1837 to 1881, though only definitely devoted to that purpose in 1855, have now been published in eight volumes, appearing at intervals from 1887 to 1902.

Groombridge's stars, his own epoch being 1810, were re-observed at the Radcliffe Observatory, Oxford, with epoch 1845, and again towards the end of the century in Greenwich catalogues, in connection with which the re-reduction of Groombridge's original observations has recently been completed and published in 1905 by the Royal Observatory. Bessel's contribution was not confined to the reduction of Bradley's observations. His own zone observations at Königsberg fill two catalogues, epoch 1825, one dealing with stars within fifteen degrees north or south of the equator, and the other with the thirty degrees of declination immediately north of the first catalogue, *i.e.* with declinations fifteen to forty-five degrees north of the equator. Argelander of Åbo, afterwards of Bonn, continued the zone observations from Bessel's northern limit to ten degrees from the north pole, epoch 1842, and from his southern limit to about thirty degrees south of the equator. The series of catalogues regularly published at Greenwich was commenced about this time, each embracing the stellar observations at the Royal Observatory for a period of from six to ten years (the twelve-year catalogue being simply a combination of two successive six-year periods), a system introduced by Airy soon after his appointment. Many other catalogues belong also to the first part of the century, including those of Rümker at Hamburg, Santini at Padua, Taylor at Madras (recently re-reduced under the direction of A. M. W. Downing, superintendent of the Nautical Almanac), Brisbane at Paramatta, Johnson at St Helena, and others.

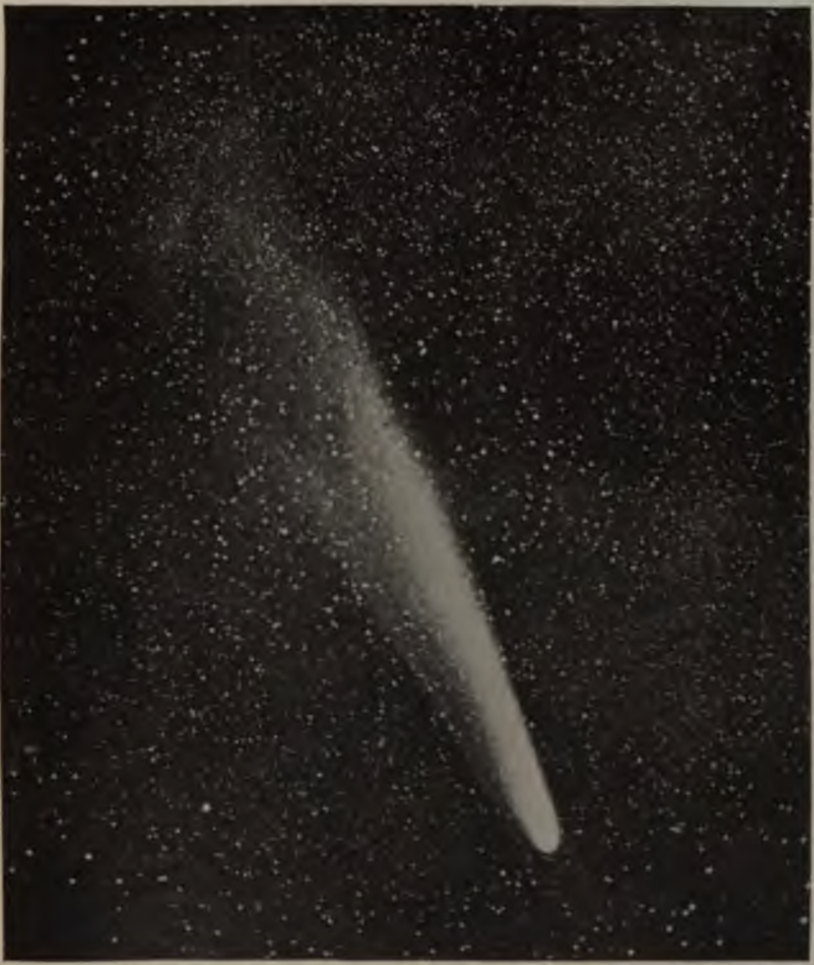
1850 was the epoch to which Lamont's Munich zone observations were reduced, and also that of the British Association catalogue, produced by the indefatigable Baily, including all stars, to about the sixth magnitude, of which reliable observations had been published, and thus forming an invaluable work of reference.

R. C. Carrington of Redhill applied himself to the observation of the polar cap north of Argelander's zone, and by his catalogue of stars from the pole to 81° north declination, epoch 1855, in addition to his valuable sun-spot observations, earned scientific recognition not often accorded to the later years of a man retiring from a commercial career.

But 1855 is most celebrated as the epoch of the Bonn Durchmusterung, carried out under Argelander and his successor, Schönfeld. It comprises in several volumes a complete survey, down to a magnitude about 9½, of the sky from the north pole to two degrees south of the equator, and to an even fainter limit for the next twenty degrees south, included in Schönfeld's continuation. The places, though not of great refinement, are very approximate; and letters are suffixed to stars identified as having been found in certain standard catalogues. This great survey, once known as A.Z. (Argelander's zones), but properly referred to as B.D., though still often quoted as D.M., despite the fact that it is no longer the only Durchmusterung, was supplemented by Schönfeld with a series of large sectional maps, showing, with meridians of right ascension and parallels of declination, every star in the B.D. No previous star atlas could compare with these Bonn maps for such purposes as the identification of a field in which a comet had been observed, often far from the region covered by the ecliptic charts of Chacornac, Bremiker, and others. They contain about 324,000 stars, exclusive of Schönfeld's continuation, or 450,000 in all.

In 1865 the Astronomische Gesellschaft, or German Astronomical Society, suggested obtaining fundamental observations of all but the faintest stars in Argelander's zones, dividing the sky into suitable portions, zones of declination, corresponding to the position and equipment of thirteen co-operating observatories. The epoch of the various resulting catalogues was fixed for 1875, but the work took a very long time to complete, and the results for the different zones are not of quite the same accuracy, the observatories, such as Helsingfors, Christiania, Leiden, Lund, Dorpat, and Washington, having, for obvious reasons, different atmospheric conditions, besides different instruments and observers.

The next step was in the direction of standardising the survey, but leading up to it was the work in the southern hemisphere: the chief catalogues produced there at the time having been B. A. Gould's Argentine General Catalogue from Cordoba observations, published in 1886, and Stone's Cape Catalogue for the epoch 1880. Stone's successor, Sir David Gill, who only recently retired from his post at the Cape, was struck with the clearness of the images of stars on photographs of the great comet of 1882, and being anxious to extend Schönfeld's zones to the south pole determined to employ photography for the purpose. The result was the Cape Photographic Durchmusterung, the photographic plates being taken in four years, and reduced to the form of a catalogue in about fourteen years by the labours principally of J. C. Kapteyn, of Groningen. But not long after the commencement of the Cape survey, Gill suggested the extension of the principle to the whole sky under international auspices; and as the brothers Henry, at Paris, had recently successfully employed photography in the extension of the ecliptic charts so necessary for the identification of minor planets, the proposal was welcomed in France, and an international conference held at Paris in 1887, at which it was decided to construct a chart and catalogue of the whole of the sky, the catalogue to include stars down to the eleventh magnitude, the charts to the fourteenth magnitude, each two degrees square, all to be taken with similar instruments and similar exposures, and the sky for this purpose to be divided into zones of declination, distributed among the co-operating observatories. The instrument of the brothers Henry was taken as the standard: of an aperture of thirteen inches, with a guiding telescope of ten or eleven inches aperture. The distribution of zones was made with regard to the resources of the several observatories, and arrangements made to obtain fundamental observations of sufficient stars to secure the accurate



GREAT COMET OF 1882. (GILL.)

reduction of the photographic observations. Thus to Greenwich Observatory was allotted the zone from the north pole to a distance of 26 degrees from it, the authorities undertaking the actual taking of the plates, more than a thousand in number, the accurate measurement of them, the production of copies, the publication of measures, and also the construction of a fundamental catalogue containing places of all the stars in the zone necessary for the determination of the plate constants. This catalogue forms the greater portion of the forthcoming nine-year catalogue, the result of a large number of observations made in the years 1897-1905. This is only one instance, different arrangements being made at different stations: for instance in the case of the Oxford zone the photographs were taken and measured at the University Observatory, Oxford, while the fundamental places were determined at the Cambridge Observatory, and the same zone is now being re-observed at Greenwich; again, one measuring bureau deals with plates from both Sydney and Melbourne Observatories, and successive conferences have had to deal with changes in the original plan caused by delay in those southern zones rather rashly undertaken by observatories whose zeal outran their financial discretion. But the work is well advanced, most of the photographs are taken and some of the volumes of results published. There is no room here for further reference to the various devices for measuring the plates and determining star-places from them; but it seems quite likely, from a comparison made between plates taken at different observatories, that owing to the slightly different adjustment of the various object-glasses the resulting survey may not be quite homogeneous.

One of the many valuable contributions of the Harvard College Observatory and its able director, Professor E. C. Pickering, is a continuous systematic survey to the sixth magnitude, carried on almost automatically by means of a photographic telescope which exposes plate after plate to successive portions of the sky in series after series, the plates once developed being indexed and stowed away. Any question of an unfamiliar object, such as a "new star," is at once referred to Harvard, where a series of plates of the region concerned, taken at different times for a long period, gives evidence of exactly the kind required. In the case of Nova Aurigæ, for instance, several plates were found to have registered its image six weeks before its "discovery." The Harvard College Observatory, with its southern outpost at Arequipa in Peru, is certainly an institution that astronomers could ill spare; neither can they fail to appreciate the spirit in which its director, having had placed at his disposal for use in the most effective manner the projected Bruce 24-inch telescope—a great advance on anything of the kind at Harvard—resisted the temptation to have it installed there, and set it up in the purer air of Arequipa, where it could be applied to the comparatively neglected field of the southern sky.

Since the middle of the century catalogues of the old kind have been fairly numerous, Melbourne and the Cape, Greenwich, Armagh, the Radcliffe, Glasgow, and other observatories, contributing British catalogues, and American and Continental institutions also performing their share; but the growing importance of the photographic method has emphasised the necessity of a different sort of fundamental catalogue, on the lines of that of the British Association. Newcomb's "Fundamental Catalogue of Standard Stars," published in 1899, is of this kind, embodying weighted mean results from all standard catalogues from Bradley's time, reduced to a common epoch by the employment of revised constants. Professor Auwers has projected a similar work for the epoch 1910, which, though completed, is at present almost inaccessible; while in 1903 Professor Lewis Boss, of Albany, published another, in which he rejected the older observations of Bradley, Mayer, Piazzi, and Groombridge, as being of insufficient accuracy for his purpose. The new century saw a new project started in Germany to collect into one great catalogue the results of all known catalogues, some three hundred in number, and as many observations as could be found not included in any catalogue, such as, for instance, those of Dr Hornsby, an old Radcliffe observer at the end of the eighteenth century, and some of his successors, whose observations have never been published. This monumental work is being carried out under Dr F. Ristenpart, and should prove of enormous value. The re-observation of catalogues is of extreme importance, especially after a long interval, as it not only enables corrections to be made to adopted values of precession, nutation, etc., but furnishes materials for the determination of apparent "proper motion," from which can be found the motion of the solar system in space, and ultimately the actual proper motions of the stars. The distance between the several epochs being a factor entering directly into the question, it is obvious that in general the oldest reliable observations must be used for the purpose; and so for many years recurrence was had to Bradley's observations, as the first point of measurement, but of late years the increasing accuracy of observed places tends to discount the necessity of a long interval between the epochs, and to enlarge the scope of the investigation to include fainter stars not observed until long after Bradley's time. Much valuable work of this kind was done by Auwers in connection with his re-reduction of Bradley's observations, and also by Porter, of Cincinnati, and Lewis Boss. The first star found to have a really large proper motion was 61 Cygni, noted by Piazzi in 1792, as having an annual displacement large enough to carry it across a space equal to the moon's diameter in less than four centuries. The next "runaway," Groombridge 1830, first noted by Argelander in 1842, long held pride of place, requiring less than three centuries to cover the same space, but a much fainter star was discovered in 1897 by Kapteyn and Innes from the plates of the Cape Photographic Durchmusterung to be moving at a pace that would do the distance in rather more than two centuries; and this star, under the appellation of Cordoba Zones, V. 243, is now the recognised "champion sprinter," from the point of view of apparent proper motion. But it must be borne in mind that, to deduce real from apparent motion, two more things are necessary in addition to the observed displacement. One, the distance, is the objective of the hunt for stellar parallax; the other, the direction of motion, is perhaps not so obviously necessary; but it stands to reason that a star moving directly towards the solar system would show no cumulative displacement whatever in a century, however fast it moved, so that small "proper motion" is no actual criterion of small velocity. We have seen, however, that by means of the spectroscope the velocity in the line of sight can be measured by the displacement of spectral lines, and by composition of the observed displacement at right angles to the line of sight, and the velocity indicated by the spectroscope in the line of sight, it is possible, if the distance be also known, to determine all the circumstances of the actual motion. It must suffice here to note that, so far as at present determined, the real velocities of the stars are in a very different order, Arcturus having an observed velocity of more than 250 miles per second, Groombridge 1830 being 100 miles per second slower, and the "champion sprinter" having less than a third of the velocity of Arcturus; these three, numbered one, three, and six in order of tangential velocity, being respectively twenty-first, second, and first in order of "proper motion."

The historic problem of stellar parallax is an application of the ordinary question in surveying, "to determine the distance of an inaccessible object," with very strict limitations on the choice of a base-line, and exceedingly small differences of angle to be measured, necessitating very great care and accuracy. The displacement of an object five miles away, viewed alternately with the right and left eye without moving the head, is greater than the parallax of any star. We have seen how many advances in astronomy are owed to the persistent search in this direction, from Bradley's discovery of aberration and nutation to Herschel's double stars, and how the genius of Fraunhofer at length provided optical means sufficient to enable Bessel to attack the problem with success. Since then the number of accepted parallax determinations has steadily increased. Gill and Elkin, with the Cape heliometer, have shown the refinement possible in the way of precautions to avoid systematic error, and Gill's determination of the parallax of α Centauri as three-quarters of a second of arc, establishes that celebrated star as the nearest neighbour yet known to the solar system. The latest production in this field is a determination, under Elkin's direction at Yale, of 163 separate parallaxes. But the photographic method, introduced by Professor Pritchard at Oxford, has obvious advantages, and is gradually being adopted at Cambridge Observatory and elsewhere, though most of the well-determined "large" parallaxes are due to the heliometer in the hands of Gill, Elkin, Bruno Peter, and others. There is also a spectroscopic method by which the distance, in the case of binaries, can be inferred from the actual velocity (from line of sight observations).

and the orbital motion, or apparent angular velocity, allowing for the inclination of the orbital plane.

A few years ago while working on the reduction of the Cape Photographic Durchmusterung, Professor Kapteyn suggested a wholesale method of parallax hunting in connection with a Durchmusterung to be undertaken expressly for the purpose. A plate was to be exposed to each field in the sky at three successive maxima of parallactic displacement and not developed until after the third exposure, the interval between each exposure being naturally six months. Thus the first and last exposures would betray any cumulative effect such as proper motion, leaving the parallax to be determined directly by comparison of the second set of images with the mean of the other two. There are, however, considerable difficulties in carrying out this ingenious scheme, which has nevertheless been actually tried on a few plates. Changes in the film, however carefully kept, cannot be guaranteed not to take place. Moreover, it was found that the exact carrying out of the scheme involved, in general, exposures alternately east and west of the meridian, causing effects undistinguishable from those of a real parallax, so that to avoid these it was necessary to expose plates only on the meridian and so to sacrifice the times of maximum displacement. There is no doubt, however, that some such plan is of great excellence in picking out stars whose parallax is large, so as to make a list of objects worth studying for the purpose.

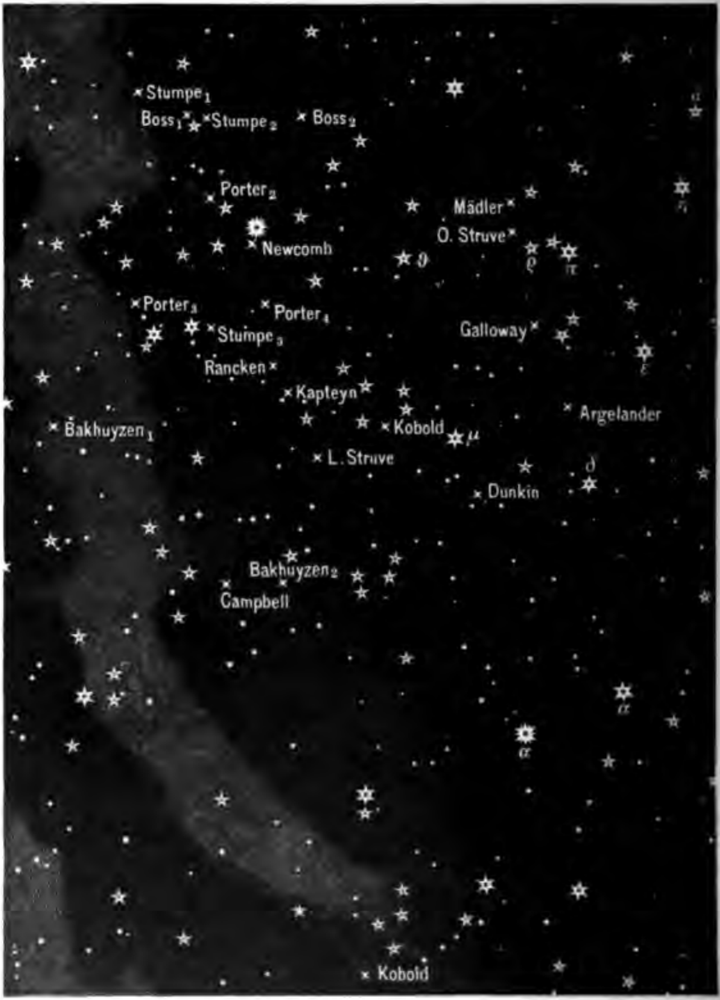
It was for a long time assumed as a working hypothesis that bright stars were nearer than faint ones, although this involved the tacit assumption of a most improbable actual equality in the stars. But parallax work on this line is disappointing, a far better criterion being apparent velocity. A faint star with a large proper motion is far more likely to show a measurable parallax than a much brighter star which does not show proper motion. The seven apparently brightest stars in the heavens are Sirius, Canopus, α Centauri, Vega, Capella, Arcturus, and Rigel; but of these Sirius shows only half the parallax of α Centauri, Vega and Capella barely a tenth, Arcturus only a thirtieth of the same amount, while Canopus and Rigel show none whatever. On the other hand more than one star of the seventh magnitude or fainter, shows nearly as great a parallax as Sirius. An obvious deduction is that so far from being even approximately equal the stars cover an enormously wide range. Canopus must be at least 70 times as far away as α Centauri, whose light reaches us in 52 months. It is inferred that Canopus gives more than 20,000 times the light of the sun, while a star of 8½ magnitude, also in the southern sky, turns out to be some 300 times less bright than the sun, giving between these two stars a light ratio of 6,000,000.

Still the connection between brightness and distance is not entirely fallacious. It has been determined, by observations with the heliometers of Yale and the Cape, that the "average" parallax of stars of the first magnitude is one-tenth of a second of arc, corresponding to a distance of 33 "light years." Similarly the average distance of second magnitude stars works out at 52 light years, of third magnitude at 82 light years and so on, the ratio between the successive distances being about the same as it would be if all the stars had the same actual brightness, for the ratio of the brightness of one magnitude to the next is 2.512 (whose logarithm is 0.4), so that the distance ratio would be the square root of this, 1.585, practically the same as the ratio of 33 to 52 or 52 to 82. It seems, however, from further investigations that the average ratios are not the same in different parts of the sky, for instance in the Milky Way and far away from it, and also that they differ for stars of different types.

The actual light received from all the stars is about 1 per cent. of the light of the full moon, and it has been proved that owing to the steadily increasing number of stars as we go lower in the scale of brightness, each successive class gives more light than the one before it. This has never been controverted, although it involves the apparent paradox that nearly all the light of the sky comes from the parts where no stars are visible to the naked eye. The difficulty is, of course, the confusion of the senses between quantity and intensity. A million candles arranged uniformly in different directions at the same distance from an observer would be all separately invisible at a distance at which one light of a thousand candle power would be conspicuous, though actually giving only one-thousandth part of the light of the million candles.

The increase of total light from each successive drop in the magnitude of the stars considered must have a limit, since otherwise there is a *reductio ad absurdum*, for the sum of an infinite series of increasing quantities must be infinite, and we might expect the whole sky to blaze with an overpowering brilliance against which the sun would be inconspicuous if not dark.

There does seem to be a change in the ratio of increasing frequency of stars somewhere below the ninth or tenth magnitude (Newcomb says at least the eleventh), but it varies in different parts of the sky. If space were filled with equal bodies evenly distributed, the number of stars visible in any direction down to a particular magnitude would be about four times the number down to the next higher magnitude, but there is no known area where the ratio is so great as this. These considerations belong rather to stellar distribution than to photometry, but before fixing our attention on advances in that direction, it will be as well to emphasise some of the difficulties attending the approved numerical results of stellar analysis. The motion of the solar system in space has been already referred to as regards direction, and various determinations of the velocity ranging from 14½ to 10 miles a second have ultimately yielded to a spectroscopic result of 12½ miles per second. The spectroscopic inquiry into the actual direction will not be complete until the result of the D. O. Mills expedition to Santiago (Chili) to extend the investigation to southern stars is published. It is, however, probable that the final result



THE STAR APEX

for the apex will not be very far south of Vega, in conformity with a note in a previous chapter.

The accompanying map from Kobold's "Bau der Fixsternsystems" shows how widely various estimates have differed.

The velocity computed for most stars for which results have been obtained averages over twenty miles per second, so that our sun is comparatively slow. But it has been computed that the "whole universe" cannot by gravitation control a velocity exceeding twenty-five miles per second, so that many stars, headed by Arcturus with a velocity ten times as great, present anomalies whose explanation has hardly been attempted. Newcomb's figures, however, are not really based on the "whole universe," which is an unknown quantity; but on the effect of 100,000,000 suns, each of five times the mass of our own, distributed over a disc-like space of 30,000 light-years in extent. A spherical space of about one-fifth of this diameter, strewn with double the quantity of gravitating matter in the form of suns, would suffice, in Lord Kelvin's view, to produce velocities ordinarily met with among the stars.

These estimates do not differ greatly from the hypothetical numbers suggested, by a comparison of photographs, for the total of the stars, 100,000,000 having been actually mentioned as a fair estimate of the probable number that would be revealed by the greatest modern telescopes.

The high velocities of Arcturus and other stars would seem to suggest a much greater quantity of unsuspected matter in the universe, possibly non-luminous or only faintly shining. Stellar photometry, however, though, as we have seen, the magnitude of a star is no actual criterion of its distance or size, is a branch of growing importance. After the time of Al Sûfi's catalogue, many ages elapsed before a catalogue appeared in which photometry was the chief feature. B. A. Gould's "Uranometria Argentina," giving the magnitudes of over 8000 stars visible at Cordoba, was published in 1879. A few years later appeared Pickering's "Harvard Photometry," of more than 4000 stars, from observations with the meridian polarising photometer; and Pritchard's "Uranometria Nova Oxoniensis," of nearly 3000 stars visible to the naked eye from the North Pole to ten degrees south of the Equator, observed with the wedge-photometer at the Oxford University Observatory. Both methods ultimately depend on the faculty of the human eye for detecting equality of light, the amount of rotation of the polariser in the one case and the thickness of the absorbing wedge in the other, to bring about the required diminution of the bright object, being the actual observation. The Harvard Photometry has been extended southwards to the other pole by Professor Bailey's observations of nearly 8000 southern stars at Arequipa, the southern outpost of Harvard College Observatory. Professor Pickering's more recent "Photometric Durchmusterung," from nearly half-a-million observations at Harvard, comprises photometric determinations of all stars down to the 7½ magnitude between the North Pole and 40° south declination. Small sources of uncertainty must always remain in visual estimates by various observers, and for the purposes of the Astrographic Chart some method of photographic photometry was required. From among the uncertainties of definition and of plates, some general formulæ have been extracted, giving a connection between the size of the image for a definite exposure and the photographic magnitude of the star, which, owing to the greater chemical action of the blue or violet rays, does not always agree with the visual magnitude. But the formulæ are very irregular and inconsistent; Pickering's device of photographing two images of the same star—one direct and the other reduced by a screen—promises more consistent results. It is to be applied to a catalogue of 40,000 stars to the 10th magnitude, and a repetition of the determinations of the first "Harvard Photometry" is promised by the new method in addition.

The Draper Catalogue has a spectrophotometric method, the intensity of a particular line being employed as the index for each star; but this is far from being a safe clue to either the visual or the photographic magnitude, especially for stars of varying types.

Even to the eye, contrast of colours, the simplest evidence of varying type, renders photometric comparison difficult; but standardisation of colour has not made much progress, as the colour perception varies in different individuals. The most important group of definitely coloured stars is that of red stars, among which Ptolemy and others of the ancients appear to have included Sirius,[1] which is certainly not red now. Al Sûfi, many centuries later, omitted Sirius, but included Algol, so even thus early there was evidence of colour changes. Lalande in 1805 gave a list of thirty-three red stars. In 1866 Schjellerup published a catalogue of 280 red stars, followed in 1876 by Birmingham with another containing 658 stars. Many of these are declared by Chambers to be rather orange than red, and it is certain that very few are of a deep red colour. In 1888 Espin published a new edition of Birmingham's catalogue, with more than double the number of stars, adding also the spectral types to which they belonged; and, five years later, Krüger of Kiel brought out a catalogue of more than 2000 coloured stars.

Antares is the reddest of the bright stars, and is said to have derived its name from having been mistaken for red Mars (anti = instead of, Ares = Mars), but most deep-red stars are invisible to the naked eye, and a great number of them are variable and appear more orange at their brightest. There are many modern instances of changes of colour like that ascribed to Sirius, including some which have apparently gone from red to blue.

Ball's cursory parallax-hunting at Dunsink, though unsatisfactory, supported the inference that red stars are not specially near our system, but they do seem to congregate in certain regions and to be often associated in pairs. This tendency is not confined to red stars, but is found in many groups in the sky, the late R. A. Proctor having collected evidence of what he called "star-drift" in several regions, one including five of the seven stars in the Plough. But one particularly interesting phase of colour observations is associated with binary stars, the next subject that comes under our observation, postponing the general question of stellar distribution.

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[1] Schiaparelli argues that the word used did not necessarily mean "red," but simply "bright."

# CHAPTER XXIX

## DOUBLE STARS

SIR W. HERSCHEL'S search for parallax resulted, as we have seen, in opening the field of double-star astronomy, and providing evidence to prove the extension of Newton's laws beyond the solar system. The field, once opened, was penetrated by Sir John Herschel and his friend South, and systematically explored by F. G. W. Struve, whose fame stands very high in double-star astronomy. His Dorpat work was carried on after his departure by Maedler, and that at Pulkowa by his son, Otto Struve, whose double stars, though less than one quarter as numerous as the 2640[1] pairs ultimately catalogued by his father, include many difficult and interesting objects, one of which, δ Equulei, has lately been proved to revolve more rapidly than any
See p. 301. other acknowledged telescopic binary. In this case, as in some others, his discovery took the form of resolving one component of a previously-recognised pair, thus forming a triple system. He was in the habit of applying to all his observations corrections of the nature of personal equation, depending on the separation and inclination to the vertical of the

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[1] W. Struve's original catalogue included more than 3000, but he afterwards rejected many of them.

components observed, having experimented with artificial double stars in order to construct a table of corrections. This cautious principle seems to have been applied in his case with only partial success.

Meanwhile in England, after Sir J. Herschel's departure to the Cape, double-star observers were not wanting, the best known being the "eagle-eyed" W. R. Dawes and Admiral Smyth. In addition to searching for new pairs, old pairs were remeasured for the determination or improvement of their orbits, and to this branch Baron Dembowski of Milan devoted more than a quarter of a century; while many names well known in other branches of astronomy will be found in the lists of those who have paid more or less attention to the measurement of double stars, as Bessel and Schiaparelli. About 1870 even Dawes thought that the field of double-star discovery was nearly exhausted, though the gradual increase in optical power available seemed to hold out some promise of new ground to be broken; but the richness of the field still remaining was manifested without that aid, for in his spare time from 1871 to 1882 S. W. Burnham of Chicago with a 6-inch telescope discovered hundreds of new double stars. His marvellous powers soon found opportunity for work with larger telescopes, such as the 18½-inch at Dearborn. Then from 1888 to 1892 he worked at the Lick Observatory, frequently using the great 36-inch for his observations, and was afterwards attracted to the new Yerkes Observatory of the Chicago University by the possibilities of the

40-inch. One of the first publications of the Yerkes Observatory was a collection of all known observations of Burnham's 1290 discoveries.[1]

The Lick telescope has not been idle, and with it W. J. Hussey and R. G. Aitken number their double-star discoveries by the hundred, as also does G. W. Hough of the Dearborn Observatory. T. J. J. See at Flagstaff, and R. T. A. Innes at the Cape met with similar success in southern fields, the latter having published a reference catalogue of southern double stars. One of the most important double-star publications of the Lick Observatory, issued in 1901, was a complete catalogue and discussion of all the known observations of the pairs of Otto Struve. The system at Greenwich Observatory does not lend itself to the search for new pairs, but the much-needed re-observation of known pairs has been regularly prosecuted there since 1893, and more especially since the mounting of the 28-inch equatorial a year or two later, under the charge of T. Lewis, who, in addition to taking most of the observations for the first few years and an important share afterwards, which still continues, has devoted himself for several years to the preparation of a full discussion of all the observations of Struve's 2640 pairs, a great work which was published last year as a memoir by the Royal Astronomical Society. In this, as also in the Lick and Yerkes publications, much space is devoted to graphic determinations of the apparent orbits of stars which have been sufficiently observed to warrant it, and many interesting facts emerge from the discussion, one of which is to refute the old hypothesis that brightness and mass necessarily go together, for in many cases the mass of the fainter of two mutually revolving stars exceeds that of the brighter to a very marked extent, and even when this is not actually the case the ratio of masses is out of all proportion with that of brightness. For instance, Burnham's close pair, 85 Pegasi, has a light ratio of 40 to 1 and a mass ratio of 1 to 6; while Procyon's companion, giving only $\frac{1}{21000}$ of the light, is of $\frac{1}{5}$ of the mass of its primary, the companion of Sirius showing a similar disproportion. These last two stars, Sirius and Procyon, the two dog stars, are remarkable instances of fulfilment of prediction based on the application of the Newtonian laws to the distant regions of the universe; for Bessel, who died too soon to see the fulfilment of his prophecy in regard to Neptune, also attributed the observed orbital motion of Sirius and Procyon to invisible companions. Six years after his death Dr Peters deduced for the hypothetical companion of Sirius an orbit with a period of about fifty years, and in 1862 Alvan G. Clark, while testing an 18-inch objective in his father's workshop at Cambridgeport, by setting it on Sirius, detected a faint star in the exact place computed by Professor Safford from Peters' theory. Auwers immediately upon this computed a period of forty years for the hypothetical companion of Procyon, which was not discovered until 1896, when Professor Schæberle caught sight of it with the great Lick telescope. Struve calculated that between two and three per cent. of the stars down to the ninth magnitude are not single, or five per cent. of the brighter ones; but later researches in this and other directions tend to show that the proportion is very much greater, though much of the evidence for this is spectroscopic, and will be referred to in its own place.

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[1] He has added a very few since that time.

Struve also paid particular attention to colour, and although in some cases difference of colour seems to vary with the closeness of a pair in its orbit, in which case it can be attributed to an optical effect, yet in general it probably indicates different stages of development, and is a safer guide to difference of mass than the broken reed afforded by difference of brightness.

Asaph Hall, the discoverer of Deimos and Phobos, also observed double stars with the great Washington telescope, as also did Barnard at the Lick and Yerkes Observatories, but a list of prominent observers would be too long for a work of this kind. Not so long is the list of those who have done much work in computing orbits, including in addition to those concerned in the recent discussions of Struve, Otto Struve and Burnham stars, such double star observers as Glasenapp of Russia, Doberck of Hong Kong, and T. J. J. See, whose book on stellar systems, published in 1895, endeavours to trace the evolution of double stars on tidal principles analogous to those often postulated for the solar system.

One pregnant suggestion made by Lewis in the great Struve memoir has a direct bearing on probable stellar distribution. He says the proportion of stars showing relative motion is in all probability much the same everywhere in space; so that if in any particular direction this does not seem to hold, the inference is not a change in the proportion, but an increase in the average distance in that direction, which prevents the detection of the proper number of relative motions, the limit of the resolving power of the telescope being directly dependent on the distance. Lewis infers that the earth is not in the centre of the visible universe.

Another feature brought out clearly in the Struve memoir is the necessity, recognised long ago by Struve himself, of accurate observations with meridian instruments. The appeal to the Greenwich Transit Circle observations in the case of such a star as ζ Herculis brought evidence of the greatest value to the discussion of a system of more than two bodies, for which it was of importance to know the order of the masses, to which relative motions gave no clue.

We need not consider multiple systems separately. An increasing number of so-called binaries are really multiple systems, as we have already noted in the case of δ Equulei; and to this class ι Hydræ, ζ Herculis, and many others belong, so much so that it is quite a usual artifice to account for apparently irregular orbital motions by the hypothesis of a close or faint companion to one or other of the components. The test of common "proper motion," applied to confirm or disprove the physical connection of a pair of stars, will not discriminate between what are usually understood by binary or multiple systems, and what Proctor called "star drift," except in cases where the proper motion is large, as, for example, 61 Cygni, which, though recognised as a double by Bradley, showed relative motion so little curved that even Burnham pronounced against the existence of physical connection. The evidence collected by Lewis in the Struve memoir seems conclusive, however, as to the actuality of the connection, though quite inadequate in a century and a half to determine the orbit.

There is another class of binary stars in general quite distinct from so-called "visual" binaries. The "astronomy of the invisible," whose study was commenced by Bessel, and to which we have already referred in connection with Sirius and other stars, is not the only direction in which the existence of bodies not actually seen can be inferred. The vast extension of the field of physical research made possible by the spectroscope, which we have already noted in connection with stellar motions and distances, again becomes prominent. There was no reason to suppose that κ Pegasi, whose period of about eleven years, until Hussey's recent research into the orbit of δ Equulei, was long considered the shortest of any known binary, really represented the lowest limit in the universe. Twin suns, so far as any evidence could possibly be adduced, might revolve much closer together than any system that could be separated in even the largest telescope, and as a case in point, the "demon star" of the Arabs, Algol, was regarded as an eclipsing binary;

and its period, of less than *three days*, indicated a proximity out of all proportion with those of stars whose revolutions are measured in years. Confirmation of the theory of Algol was forthcoming as soon as the line-of-sight velocity was investigated by Professor Vogel, showing motion alternately towards and from the observer in a single bright body, periodically dimmed but otherwise unaltered in quality of light, proving that the revolution of two bodies, one dark, suggested by Goodricke in 1783, did actually take place so nearly in the plane passing through the earth that the bright body suffered partial eclipse at each revolution. This evidence, produced in 1889, was not the first example of a star being proved double by the spectroscope.

Dr Henry Draper, whose work in solar spectroscopy has already been noted, commenced a spectroscopic investigation of stars, cut short by his death in 1882. But in 1886 the Draper Memorial work was commenced at Harvard College Observatory under Professor Pickering, with instruments and funds provided by Mrs Draper, and its first publication, which appeared in 1890, took the form of a catalogue of the spectra of the brighter stars in the northern hemisphere. The second Draper catalogue was to contain the spectra of 30,000 stars in both hemispheres, the instrument used at Harvard having been sent to Arequipa in 1889 to secure uniformity of results. The work of measurement and discussion was in the hands of a staff of ladies under Mrs Fleming; and it was found on comparing several spectrograms of the brighter component of Mizar, ζ Ursæ Majoris,[1] the middle star in the "tail" of the Great Bear, or the "horse" on which the "waggoner" rides in charge of Charles' Wain, that two of them taken in 1887 and 1889 showed the K line double. Professor Pickering announced this discovery, indicating his conclusion that the evidence pointed to the star being double. He entrusted Miss Antonia C. Maury, a niece of Dr Draper, with the examination of seventy spectrograms, which showed that the other lines in the spectrum were also affected. It seemed then that two nearly equal bodies mutually revolved in a plane not greatly inclined to the line of sight, and that the doubling of the lines must indicate velocities in opposite directions in the line of sight; while single line spectrograms would correspond to times when the motions (though still, of course, in opposite directions) were practically across the line of sight, betraying no measurable radial velocity. This was the first discovered spectroscopic binary, though the principle employed had been suggested for investigation, though not for discovery, by Fox Talbot in 1871. Miss Maury worked out an apparent period of 52 days between successive widenings, indicating a period of revolution of 104 days, but this has been reduced by Vogel in 1901 to rather under 21 days, the uncertainty being due to an orbit far from circular.

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[1] Curiously enough, Mizar was also the first recognised visual double star, having been noted at Bologna by Riccioli in 1650, and also the first photographed as such by G. P. Bond in 1857; moreover there is every probability that Mizar with Alcor was the first, as it is certainly the best known, "naked eye" double star, the names having been given by the Arabs.

In 1889 Miss Maury detected a four-day period in β Aurigæ, which thus ranks as the second discovery of the new class. It has been computed by Huggins that this binary could not have been discovered visually, as to separate the components would require a telescope of 80 feet aperture.

In 1890 Vogel added another, Spica, to the list, but in this case the duplicity was betrayed, not by the doubling of the lines, for the companion appears to be dark, or too faint to show a spectrum, but by their oscillation, denoting motion alternately towards and from the earth. Both components of Castor appear to be spectroscopic binaries, the fainter one announced by Bélopolsky, of Pulkova, in 1896, the other recently by H. D. Curtis, of Lick Observatory. In 1899 Capella, almost the brightest star in the northern hemisphere, was simultaneously announced by W. W. Campbell, of the Lick Observatory, and H. F. Newall, at Cambridge, to be a spectroscopic binary, with a period of 104 days. The components appeared to be probably not very unequal in brightness, and, the parallax being known to be sensible, it was computed that the apparent angular distance might be not much less than a tenth of a second of arc, so there seemed a possibility that the star might prove to be a connecting-link between "visual" and "spectroscopic" binaries, and it was carefully scrutinised with some of the most powerful telescopes. The Lick telescope betrayed nothing, the image appearing quite round at all times, but the Greenwich 28-inch, having its object-glass adjusted for a different part of the spectrum, yielded a more promising result, and though many of the observations published are probably worthless, and may be ascribed to optical or psychological causes and to want of experience in judging of the suitability of observing conditions for what was practically a unique observation, yet there is little doubt that a certain proportion of the observations can be regarded as genuine. The mean resulting period from several revolutions in successive years comes out persistently between 103 and 104 days, the spectroscopic determination being exactly 104 days, and the discordance seems to suggest a motion of the apse line of the orbit of Capella, of which the spectroscopic observations would take no cognisance. No other instance is yet known of a spectroscopic binary affording much chance of visual confirmation, though a period of about three years in β Capricorni, for instance, is a promising sign.

The number of recognised stars of the new class is steadily increasing as the determination of velocities in the line of sight reveals more and more instances in which that velocity is variable, indicating orbital motion. By the end of 1904 the number had reached 140, most of them being of the same character as Spica, the second star in point of brightness being dark or very faint. These 140 stars, with four others discovered after Jan. 1, 1905, are given in the first catalogue of spectroscopic binaries issued by the Lick Observatory in 1905. When we bear in mind that the probably large number of binaries whose motion is nearly at right angles to the line of sight cannot be detected by this method, it is not difficult to see how far from rare is the occurrence of physical pairs.

# CHAPTER XXX

## VARIABLE STARS

WE have noted the gradual widening of the field of variable star observation, from the isolated "new stars" of Hipparchus and Tycho Brahe to the discovery of variables of long and short periods, of Mira, of Algol, and of the anomalous η Argûs, and incidentally remarked on the binary character of eclipsing variables like Algol, indicating the spectroscope as a valuable aid in this as in other fields of investigation. It must suffice for the earlier period, when this branch was of very slight importance, to note that the increase in photometric accuracy realised by Argelander in preparing the Bonn Durchmusterung, in all cases insisting on grading not in "magnitudes" but in tenths of a magnitude, was responsible for the quicker recognition of smaller variations hitherto unnoticed, and indirectly for the attraction of many enthusiastic workers into a field suddenly become one of great promise, and not requiring necessarily great optical power. It was not until 1880 that Pickering published a classification of variables, according as their variation is—(1) non-periodic, (2) great and slow, (3) irregular, (4) quick and in general small, or (5) of an eclipse character. It has been claimed that the evidence collected by J. E. Gore of slow secular change demands a new class, but it is doubtful if any star in the universe can be considered as absolutely invariable, and a star that shows a small diminution of lustre in a period of many years, sometimes of centuries, may after all turn out to be periodic. On the other hand, others, on the ground that "variables" ought to be restricted to periodic variables, object to the inclusion even of "new stars," the chief members of Pickering's first division, and might similarly object to some members of the third class. The truth is, that it is as usual hard to draw the line of exclusion or even of classification.

The first class should include, besides new stars, such as those of Hipparchus, Tycho Brahe, and Kepler, also what are called "lost stars," of which there are some authentic instances. Failure to re-observe a few stars found in old catalogues may occasionally be explained in other ways, some being traced to then undiscovered planets, some to observation errors, such as a wrong hour for right ascension or mistake of ten degrees in circle reading, a wrong sign for declination or other errors of transcription; but it does occasionally happen that an undoubted star, such as 55 Herculis, is no longer to be seen, and must be relegated to the "temporary" class. Tycho's Nova, for a time as bright as Venus and visible in daylight, was the brightest of this class; that of Kepler in 1604, estimated at its maximum equal to Jupiter, being followed by a very long period without such a phenomenon. It is probable that a small star still visible marks the place of Tycho's Nova Cassiopeiæ, 1572. A characteristic of stars of this type is the sudden increase in brightness, compared with the slow fading away. The Nova of 1572 increased for about a month from discovery, and faded for five months before being invisible to the naked eye. That of 1604 reached its maximum in a few days, remained very bright for a month or more, and was visible to the naked eye for more than a year, disappearing early in 1606. Both these, of course, appeared before the invention of the telescope. Nova Coronæ, discovered by Birmingham in 1866, reached the second magnitude very quickly, fading also rather quickly. It was called T Coronæ, according to the convention by which all variable stars discovered in a constellation, unless already provided with names, are assigned the generic name of the constellation, prefixed by successive capital letters from R to Z; after nine have been appropriated in the same constellation, the same process is applied as in the case of minor planets, except that only the same nine letters are used, so that UY Cygni, for instance, is the eighth of the fifth series, or the forty-fourth name thus allotted to a variable in that constellation. T Coronæ was the first Nova examined through the spectroscope, and the appearance presented to Huggins was of two spectra, one similar to that of the sun, the other superposed upon it of five bright lines, attributed to incandescent hydrogen. Nova Aurigæ, discovered early in 1892 by Rev. T. D. Anderson, of Edinburgh, was proved to have been visible more than a month earlier by reference to

See p. 278. the Harvard photographs; it remained visible for a longer time, enabling continuous study of its spectrum to be made, and even after fading beyond the reach of all but the greatest telescopes, brightened up to the ninth magnitude, and continued to fluctuate for about a year longer before again fading into insignificance. At first the spectrum was double, as in the case of T Coronæ, and the deduction from it at first was the presence of two bodies, one approaching the sun with a velocity of about 500 miles per second, and the other receding at about 250 miles per second according to the gaseous hydrogen lines, or 150 miles according to the calcium lines. The hypothesis of a grazing collision between two bodies thus inferred has not been substantiated. After fading, the spectrum appeared faint and continuous, with a green "nebular" line, subsequently resuming a stellar appearance. Mrs Fleming claimed five Novæ detected on Harvard photographs before the end of the century; but the new century had not long commenced, when the discoverer of Nova Aurigæ obtained a more striking success by the recognition of a new object in Perseus, which is attributed generally to him, though possibly seen earlier by a Russian student at Kiev. Two days before discovery, it was invisible on a Harvard photograph showing stars to the eleventh magnitude, but almost immediately after discovery it was rather brighter than Capella, much brighter than any Nova since that of Kepler, nearly 300 years before. During its fading, it brightened regularly about every three days for some time, and, owing to its having been discovered some days before its maximum, the spectroscopic indications of its character were not at first of the normal type. It went through the usual series of changes, afterwards appearing as a planetary nebula on its way to become an ordinary star again after the lapse of a few years, taking three years to reach the tenth magnitude. But it had one unique feature, in the form of a nebulosity surrounding it, actually shown on a photograph taken with the Crossley reflector at the Lick Observatory within six weeks of its appearance, but not noticed until some months later. Messrs Flammarion and Antoniadi announced, six months after the discovery of the Nova, that it exhibited a nebulous aureole. Dr Max Wolf, at Heidelberg, while attributing the aureole to the peculiar character of the light, noticed on one of his photographs a strong trace of nebulosity some distance to the south, and suggested a photographic test with the great American instruments. Perrine at Lick Observatory and Ritchey at Yerkes Observatory then obtained photographs showing a growing nebulosity in the form of a series of spirals, apparently springing from the Nova and spreading outwards at the rate of 11 minutes of arc in a year, which, by comparison with the earlier Lick photograph, seemed to have started some days before the original outburst of light. As no parallax had been detected, this involved velocity of motion so enormous as to be quite incredible on the ordinary supposition that the nebulous matter was being ejected by the Nova. It was suggested by Kapteyn, Seeliger, and others, that the nebulosity was there all the time, but was becoming visible as the light from the Nova reached each successive convolution, and though the light showed no polarisation, so that it did not seem to be reflected light, yet some such velocity as that of light, far transcending any imaginable motions of particles, seemed requisite to account for the rapid growth of the nebulous appearance. The velocity of light would suffice at a distance corresponding to a parallax of twelve-thousandths of a second of arc; and very careful measures suggested a small parallax not far from that value. We may then tentatively attribute the phenomenon to electrical or other excitement propagated outwards from the centre, with a velocity something like that of light, rendering successive portions visible. F. W. Very considered that actual emission of particles by electrical repulsion, or light pressure, might possibly take place at a sufficient rate to explain the phenomenon, and the question cannot be regarded as settled.

An example of the accidental factor in discovery was afforded in the case of Nova Geminorum, 1903. Professor Turner, director of the Oxford University Observatory, picked up an astrographic plate put aside as rejected. On inquiring the reason, as the plate appeared perfectly good, he was informed "wrong setting," the usual cause of such being an accidental error in reading one of the setting circles by which the observer views a wrong field, and consequently picks up a wrong guiding star. Further inquiry, however, showed that this was not the usual case, but that the intended guiding star had been put out of countenance by a brighter one not far off, which had naturally caught the observer's eye. But no brighter one was indicated there in the zone catalogue, and it turned out that the guiding had been done by means of a Nova.

The original idea that the appearance of these "temporary stars" must be attributed to a collision between two more or less solid bodies, or one solid body and a fairly condensed nebula, is gradually being abandoned, since, of the several successive different phases shown in the spectrum of a Nova until it reaches the stage of a faint star again, there is not one that necessarily denotes instability. Even the characteristic spectrum associated with a Nova, not reached the first day or two after discovery in the case of Nova Persei, is seen permanently now in the celebrated η Argûs, and other stages belong to well-known spectral types, of which there are hundreds of examples in the sky.

Of periodic variables the number announced is rapidly increasing, though the determination of periods and light curves is a slower process. Chandler's first catalogue of variables, appearing about 1888, contained more than 200 stars. His third, in 1896, contained nearly twice as many, and as he found too little time to complete his fourth the work has been undertaken on a much more extensive scale by the Astronomische Gesellschaft. Meanwhile, however, in 1903, the Harvard provisional catalogue, with supplement, included more than 1300: 400 were detected at Harvard alone in 1904 from comparison of photographs, more than half of them in the two "Magellanic Clouds,"[1] and nearly a quarter in the Orion nebula, followed next year by more than 800 in the smaller Magellanic cloud. In the same two years practically 400 variables were officially numbered as authentic.

By far the greater number of long period variables have periods of 250 to 400 days, the first known, Mira Ceti, varying from second to tenth magnitude in about eleven months. χ Cygni varies from 4½ to 13½ magnitude in 13½ months, V Delphini from 7.3 to 17.3 in 540 days. This is the widest range of magnitude known, corresponding to an actual brightness ratio of unity to ten thousand. Mira, however, though its range is about eight magnitudes, sometimes rises to a higher maximum than usual, and sometimes sinks to a lower minimum. This irregularity in degree seems not so strange as it would be in the case of an eclipsing variable, for the spectroscopic evidence goes to show that each brightening is of the nature of a fresh outburst of activity in the form of blazing hydrogen. There seems no periodicity in the high maxima, unless we accept one of 59½ years suggested by P. Guthnick, and the light does not invariably follow the usual rule of a rise quicker than the fall in brightness. Moreover, on one occasion the maximum was a month late, while minor deviations from the mean period are not unusual. These irregularities point

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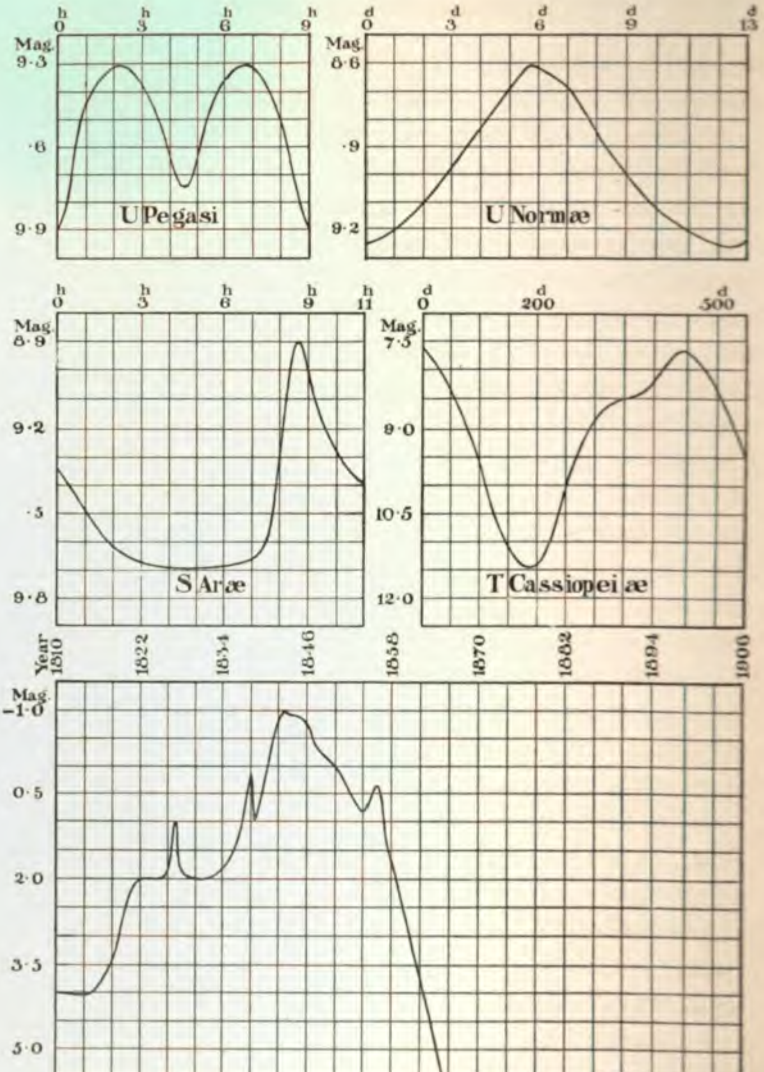
[1] Two detached objects, similar in appearance to the Milky Way, but nearer to the South Pole.



LIGHT CURVES

to an analogy with the sun-spot period, though on a very much exaggerated scale.

χ Cygni is even more irregular, and has also a greater range of magnitude. Its average period has been lengthening slowly ever since its discovery in the seventeenth century, and is now one per cent. longer than it was. Some of these stars show well-marked double maxima, and some are only regular in the fact of their alternations, no similarity showing itself between successive ranges of variation. ι Aurigæ was supposed to be quite irregular though only slightly variable, until in 1903 Dr Ludendorff of Potsdam got out a period of just over twenty-seven years, far longer than that of any other known variable. The most irregular variable known is beyond question η Argûs (more generally known since the sub-division of that great constellation as η Carinæ), to which we have alluded before as now showing the typical spectrum of a Nova. It is supposed to have been fainter than the fourth magnitude in Ptolemy's time, as he did not record it, though others at the same altitude at Alexandria are found in the Almagest. Halley, in 1677, at St Helena, obtained the first known observation of it, assigning to it the fourth magnitude. Ten years later, and again to Lacaille in 1751, it was of the second magnitude. The variation, since more regular observations began, is shown in the accompanying diagram, showing that for some time, nearly ten years in fact, it was brighter than any star in the sky except Sirius, but has been invisible to the naked eye since 1868, and now shows a dull red in the telescope with no apparent variation at all.

Red stars show a large proportion of variables, and nearly all long-period variables are red, so that there seems some connection between the two characteristics. A. Safarik of Prague, from five years' observation, 1883-1888, of a list of twenty-two red stars, found eight variable and five more or less dying, whose loss of light was confirmed from previous observations. A few cases are, however, known of red stars growing brighter and remaining so. Of the variables of short period, generally taken to mean those of period less than a month, by far the greater number go through their cycle of change in less than eight days, and a fair proportion in less than one day. All of them seem to be close binary systems revolving in the period of variation like the eclipsing variables of the Algol type, but the changes in Pickering's fourth class are not due to eclipse. They are sometimes divided into three subordinate classes, the first, of which δ Cephei is a type, containing stars showing no tendency to stay at maximum or minimum, their rise being, however, about twice as quick as their fall in luminosity. Spectroscopic evidence suggests, in every case tested, the presence of a dark or faint companion, in fact all appear to be spectroscopic binaries. A recent suggestion of Dr Meyermann in 1905 to account for the variation of δ Cephei is that the orbit is eccentric, and the light change due to a change in radiation depending on the distance. The second sub-division, cluster variables, includes those hundreds noted at Harvard in the Magellanic clouds, and very few are found outside clusters in the open sky. They generally show periods of less than a day, with a very sudden rise to maximum, a much slower return, and a strong tendency to stay at minimum. Y Lyræ, discovered by A. Stanley Williams in the northern hemisphere, and S Aræ by R. T. A. Innes in the southern, are very similar typical cases.

In 1904 Madame Ceraski discovered a cluster-variable in Cygnus with a computed period of only 3 hours, 12 minutes. In the previous year a period of about 4 hours for a star in Ursa Major had been claimed as the shortest known, only three stars having shown variations half so quick. ζ Geminorum, the main type of the remaining subdivision, is a spectroscopic binary discovered independently by Bélopolsky and Campbell in 1898; but though the companion is a dark body the minimum does not occur at conjunction, and is therefore not due to eclipse. The feature of this class is the regularity of their rise and fall, though one, S Antliæ, one of the three referred to in the last paragraph, having a period under eight hours, shows a marked tendency to remain at maximum, and so was for some time regarded as an Algol variable. The best known variable in the group is β Lyræ, one of several instances of a sort of double periodicity, equal maxima being separated by alternate unequal minima, suggesting that the appearance may be caused similarly to that of Algol variables, except that in this case the bodies, though unequal, are both bright, so that each alternately occults the other. On this hypothesis Professor G. W. Myers concludes that the smaller body is the brighter, the low minimum occurring while it is behind the larger body, the higher one when it passes in front. He also accounts for the regularity of the changes by assuming a gaseous constitution for the bodies, which are supposed to revolve almost in contact, so that by mutual attraction they are elongated. Thus the maxima occur at times when there is no foreshortening, whose effect as they rotate is to cause a steady diminution of light until conjunction. A similar case, U Pegasi, Myers considers an example of recent investigations of Poincaré and Darwin on possible figures of equilibrium for revolving masses of fluid, one of which called apioidal or bee-shaped is that of two masses in actual contact. It is this hour-glass form that Myers suggests for U Pegasi.

On the assumption that Myers is right these stars might be relegated to the Algol class, as they would owe their principal loss of light to eclipse. Professor Newcomb suggests distinguishing between stars whose variation depends on the direction of the line of sight, and those independent of it. All Algol variables would cease to be variables if the line of sight were perpendicular to their orbital planes. So also would β Lyræ and U Pegasi, according to Myers. It is very difficult to draw a line, especially as the suggested explanation may not be correct. In the case of Y Cygni, discovered by Chandler in 1886, with a period long uncertain owing to its commensurability with daylight, a suspected period of 1½ days being difficult to verify as alternate minima would be invisible, ultimately showed a variation in three days with two unequal portions of 33 hours and 39 hours respectively, indicating an eccentric orbit not bisected by the line of sight. All such stars as these would be certainly regarded as Algol binaries by A. W. Roberts, of Lovedale, South Africa, one of the leading workers in the southern hemisphere at this branch of astronomy. He distinguishes no less than five types of Algol variation, one of which, $R^2$ Centauri, is almost a model for U Pegasi, and claimed as an apioidal binary. It is evident that a dark component is not universally considered essential for an Algol variable. Roberts obtains separate curves according to the relative size and brightness of the two components. The longest period known for an Algol binary is just over thirty-one days, in the case of UZ Cygni, discovered by Mrs Fleming in 1902, which she considers probably an apioidal system. Three years later this discovery was followed in the same place by that of $W^2$ Tauri, whose variation, from approximately the seventh to the eleventh magnitude, is the greatest of any Algol-type star. Notwithstanding the range, which, in a long-period binary, increases more or less with the period, this star, like all but two of the Algol group, goes through its changes in less than five days, as a matter of fact in about 2¾ days. H. N. Russell of Princeton, and A. W. Roberts, from different data found the average density of Algol variables much less than that of the sun, and their spectral characteristics also differentiate them from ordinary short-period variables which are of the solar type. Variable stars do not appear to be near the earth. Mira has a proper motion of a quarter of a second of arc annually, but the only parallax determined by Dr Chase makes Algol ten times as distant as Sirius, or nearly ninety light-years away.

Like the closely allied red stars, many of which are variable, they seem gregarious, as besides the class of cluster-variables several are found near the positions of various Novæ, indicating that certain regions of space are more favourable to variation.

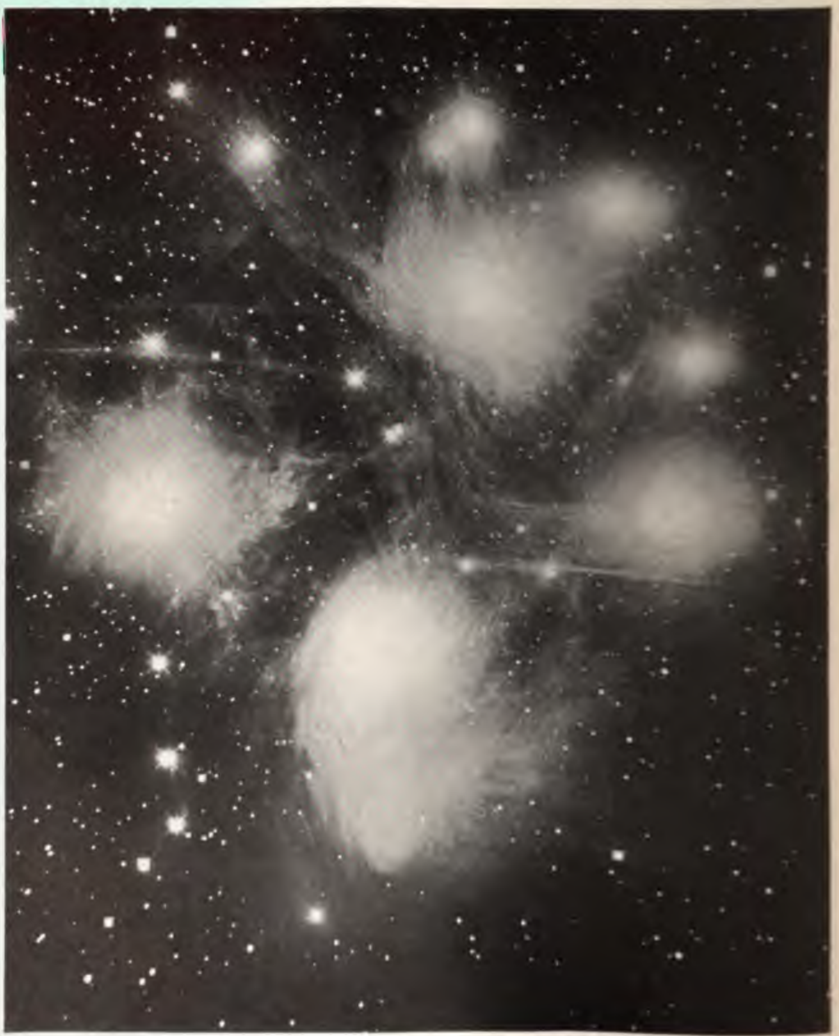
The best known observers of variable stars in this country are Stanley Williams, Anderson, and Gore, the British Astronomical Association also doing good work under the direction of Colonel Markwick. Elsewhere W. Ceraski and his wife at Moscow, Roberts in South Africa, and a large number of Americans have devoted themselves to the work, including J. A. and H. M. Parkhurst, E. F. Sawyer, and P. S. Yendell, in addition to the Harvard band under Professor Pickering and Mrs Fleming, whose investigations are mostly photographic. We have already referred to S. C. Chandler's work in forming a catalogue, now in the hands of Dunér, Müller, and Hartwig. A very valuable contribution to the subject is Father Hagen's Atlas Stellarum Variabilium, which is in constant demand among those who do the necessary work of carrying out series of observations of recognised variables, in order to confirm or improve the periods and light-curves.

The latest catalogue, the Second Harvard Catalogue of Variable Stars, contains 1791 variable stars in the Magellanic clouds, and 1957 elsewhere, showing the enormous recent progress now being made in this subject.

# CHAPTER XXXI

## CLUSTERS—NEBULÆ—MILKY WAY

THE history of research among clusters and nebulæ is to a great extent that of improvements in photography. It is impossible to dwell in any great detail on the manifold varieties among the thousands of objects indexed in Dreyer's General Catalogue of Nebulæ and Clusters, enlarged and revised from that of Sir John Herschel, and containing with supplement nearly double the number of Herschel's five thousand, indicating the progress in thirty years from the publication of the one in 1864 to the completion of the other with its supplement between 1888 and 1894. The Herschels considered nebulæ a distinct class; but the Earl of Rosse, in the heyday of his great Parsonstown reflector, resolved so many apparent nebulæ into clusters that an opinion gained very general support that all nebulæ could be similarly resolved. The year of the publication of Herschel's catalogue saw the vindication of their older theory, for Huggins then turned his spectroscope towards a nebula, and found instant evidence from its bright line spectrum that it was no group of stars but a mass of incandescent gases, which appeared to him to be hydrogen and nitrogen. Many other nebulæ gave similar results,



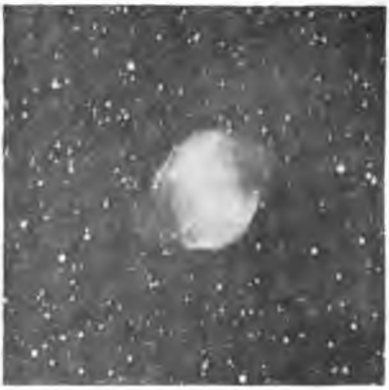
NEBULOSITY IN PLEIADES (YERKES)

though the line attributed by him to hydrogen and by Lockyer to magnesium has ultimately proved to belong to an unknown substance, to which the name of nebulium was given, just as helium had been assigned to the chromospheric substance responsible for the $D_3$ line. In many nebulæ the gas may be under pressure, as they show a continuous spectrum; or they may after all be more similar to clusters, many of which also yield a continuous spectrum. Helium itself is found in some of the nebulæ. Clusters seem to suggest universes in being, just as nebulæ may be taken to represent them in the making; but there is a marked difference between such a group as the Pleiades, in many ways the most remarkable collection of stars in the sky, and the globular cluster, of which ω Centauri is an excellent example. The Pleiades, whose rising and setting ruled the agricultural calendar of several distinct primitive nations, and which have been by others taken to be the abode of the gods, and more recently regarded as possibly the centre of the universe, the brightest star, Alcyone, being suggested as the central sun, are certainly an irregular cluster. Bessel's triangulation of the group, 1829-1841, repeated by Gould a quarter of a century later from a photograph by Rutherfurd, and very carefully by Elkin at Yale in 1884-85, shows that of the fifty brightest stars in the group, only six do not appear physically connected with it; the rest have a common apparent proper motion, almost entirely due to the motion of the sun in space, and are consequently inferred to be at a vast distance from the earth, so that every one of the forty odd stars is brighter than the sun, and the brightest of them probably 170 times as bright. Subsequent estimations by Pickering and Stratonoff show that not only the six stars above differentiated, but most of the fainter stars also, are at an enormously greater distance and do not belong to the system. It is impossible to obtain a just appreciation of the conditions in such a system as compared with our own, the dimensions being out of all proportion. Possibly the feature best worth recording is the extraordinary ramifications of nebulosity in which the stars seem to be involved, the detection of which is one of the triumphs of celestial photography.

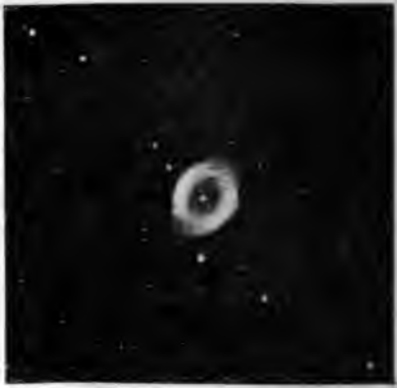
Some faint indications of a globular form have been claimed even for the Pleiades, but it is unmistakable in ω Centauri; other clusters appearing elliptical, triangular, or fan-shaped, and many groups of the stars in clusters showing apparently closer physical connection, in some cases like beads on a string of nebulosity. In an area round ω Centauri less than twice that covered by the full moon, more than six thousand stars have been photographed, of which three quarters appear to belong to the system, and 125 to be variable. A much smaller cluster, 47 Toucani, shows even more stars. Moreover, as a rule the arrangement of the stars in a cluster does not seem quite haphazard; for instance in some the brighter stars seem to represent crooked spokes, while the fainter ones crowd into the centre.

It seems very possible that clusters represent condensation from nebulæ. Globular clusters show

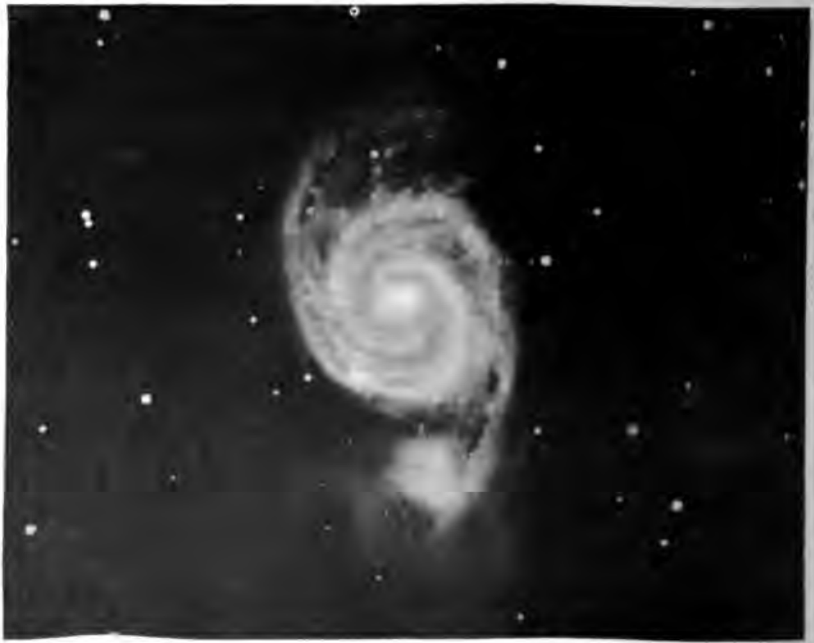
CLUSTER OMEGA CENTAURI



"DUMB-BELL" NEBULA



RING NEBULA IN LYRA

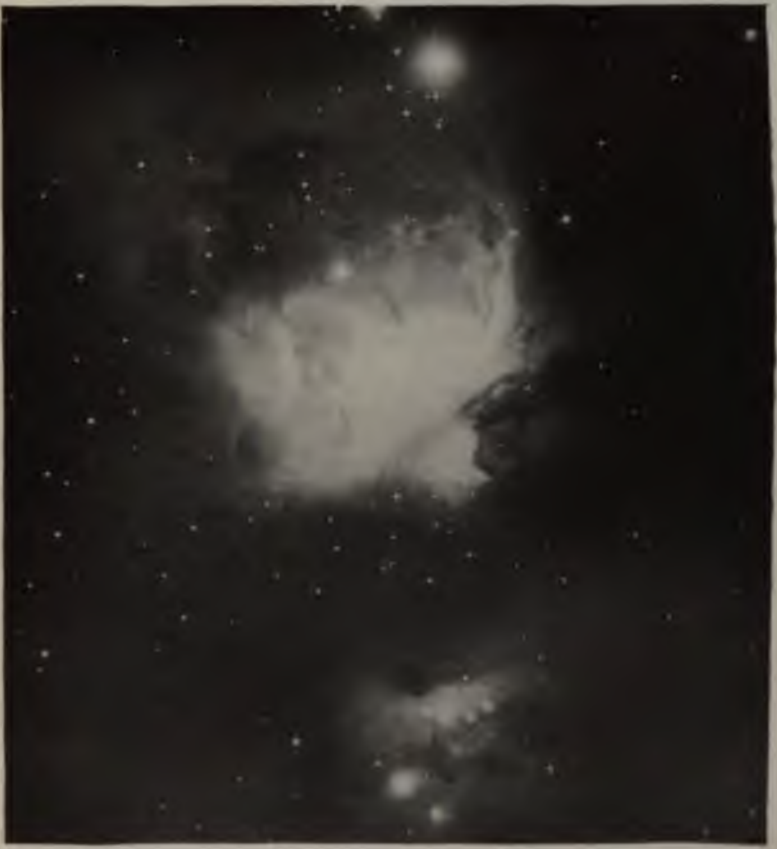


"WHIRLPOOL" NEBULA IN CANES VENATICI

no true nebulosity; the Pleiades may correspond to one intermediate stage, and various other forms to other stages. Perhaps some day the spectroscope may enlighten us further as to possible processes of evolution.

Meanwhile the nebulæ themselves show numerous gradations. The first catalogue of nebulous objects, many of them clusters beyond the resolving power then available, was produced in 1781 by Messier, the "ferret of comets" of Louis XV., who hoped by providing a list of such spurious comets to save himself some proportion of his frequent disappointments. The very names of many of the better-known nebulæ—the Ring nebula in Lyra, the Crab, the Dumb-bell, the Keyhole, the Fish Mouth, the Spider, the America nebula in Cygnus, and the Whirlpool Messier 51 in Canes Venatici—indicate great diversities of apparent shape, but other differences are more important. Nebulous stars, surrounded by a sort of glow of nebulosity, may be only perspective variants of stars with nebulous appendages of various forms, and at least one of Herschel's nebulous stars is really a close spiral. Planetary nebulæ, whose uniform brightness almost persuaded Herschel to discard them, have generally stellar nuclei also, and one of them betrays a sort of spiral form. In fact almost every kind of nebula seems to hint at some stage of the same development.

Apart from photography, the amount of work done in recent years is not great. Lewis Swift, a well-known comet discoverer, devoted a considerable time to the successful search for new nebulæ; and G. Bigourdan, of the Paris Observatory, has performed a very troublesome task in determining accurately the positions of hundreds of these objects, publishing a catalogue of nebulæ and clusters in 1899. Since 1880, when Draper photographed the Orion nebula, many reflecting telescopes, whose special advantages for the recording of faint nebulosity render them much more suitable for the purpose than refractors, have been employed in similar work. In 1883 Dr A. A. Common of Ealing obtained with his large reflector a very fine photograph of the Orion Nebula, for which he received the gold medal of the Royal Astronomical Society. In 1885 Dr Isaac Roberts began to devote himself to the task, and by the end of the century had published two volumes of splendid photographs, also receiving the gold medal. Max Wolf, at Heidelberg, and Barnard and W. H. Pickering, in America, worked in the same field, the former publishing in 1902 a catalogue of about 1500 nebulæ north of the Milky Way. The most striking success was achieved by Keeler with the Crossley reflector at the Lick Observatory. This was the 3-foot instrument made by Calver for Dr Common, from whom it was purchased by Mr Crossley, a Yorkshire merchant, who, finding the atmosphere of Halifax quite unsuitable for so large an instrument, presented it to the Lick Observatory, where it was for a time almost ignored, one and another finding it unsuitable for various purposes. On the appointment of J. E. Keeler to the director-



GREAT NEBULA IN ORION (GREENWICH)

ship in 1898, he at once justified his election by undertaking to work himself with the despised instrument, over which friction had arisen, and with rare intuition devoted it to nebular photography with such success that in the two years left him (he died in 1900), he not only found nebulæ so thickly strewn on his photographs that he estimated the number in the sky within reach of his instrument at about twenty times the number already known, inferring a total exceeding 100,000 (half a million has since been suggested by Professor Perrine), but also came to the conclusion that at least half of them are spiral, and from this produced the germ of the planetesimal hypothesis.

This idea, to which reference has already been made, does not concern itself with the genesis of nebulæ from protoplasmal material by condensation of gases or collisions of meteoric dust, but endeavours to generalise the subsequent development of all nebulæ in a manner not at variance with a plausible history of the evolution of the solar system from its supposed primitive nebula. Professor F. R. Moulton starts afresh from a mathematical point of view with a more or less spheroidal nebula, rejecting as unnecessary Laplace's assumed high temperature, in the light of modern researches into the mechanical production of heat and conservation of energy. Two such bodies in rotation traversing paths approaching near to each other will be subjected to powerful tidal effects, whose effect will be to elongate them towards and from each other, and the suddenness of the strain on bodies loosely coherent might readily cause disruption of a portion at each end of a diameter. The relatively lower velocity of these fragments will cause them to leave the parent body in a spiral direction, dragging after them wisps of uncondensed material. The continuation of this process, so long as the bodies are near enough together, is supposed ultimately to leave a central nebula with spiral arms knotted at regular intervals with more condensed masses, all rotating and gathering up the remnants of nebulosity. The evolution from this of a globular cluster differs only in dimensions from that suggested by Moulton and Chamberlin for the solar system. We cannot yet refer all the systems apparent in the sky to stages in this hypothesis, which is sufficiently suggestive to meet with provisional approval. It may be that the more condensed planetary nebulæ have been too solitary to conform to pattern, and that double stars represent cases where approach has been too near to allow of recession. It may be that the nebulosity round Nova Persei is an imperfect sketch, showing the spiral form in non-luminous matter also, revealed in some way by the light change in the central condensation. There is plenty of scope for interesting speculation in the attempt to harmonise every apparently unique phenomenon with the resistless workings of a universal law. But the hypothesis has not reached the status of Newton's law of gravitation, and may after all meet the fate of his dynamical theory of light. The source of the original energy is yet to seek, though physical research into the possibilities of radio-activity may supply a clue.

The Milky Way, stretching with hardly a break all round the heavens, though for some distance divided into two branches, suggests an obvious plane of reference in celestial distribution, and one of the first theories of the universe, the "grindstone theory" of Thomas Wright of Durham, 1750, assumes that the condensation of stars in that region indicates the shape of the universe, our system being supposed to lie in the interior of a disc-like aggregation of separate bodies, which naturally appear more thickly strewn in the plane of the disc. Kant's idea involved a plurality of such systems, regarding nebulæ as "island universes," each with its own Milky Way.

Newcomb's counting of stars in various regions points to the result that the fainter the stars considered the greater is their crowding in the Milky Way compared with the rest of the sky. Moreover, gaseous nebulæ, novæ, and stars of certain spectral types, in general the more photographically bright stars, appear more frequently in the Milky Way than elsewhere. The conclusion that the Galactic plane denotes the direction of greatest extension of our universe is almost inevitable. At the same time analysis of proper motions suggests to A. S. Eddington at least two distinct universes, so to speak, moving in different directions. A similar idea has appeared before in the guise of a suggestion from Gill and Kapteyn that the brighter stars as a whole showed rotation with regard to the fainter ones, but it apparently broke down under further analysis. It is difficult to reconcile our ideas of gravitation with interlacing and yet independent motions, but suggestions have been thrown out to the effect that such systems might persist at different distances, so that one moved wholly within the other in a manner analogous with the idea of rotation of the brighter stars (still presumed in the mean to be the nearer stars). Or again, it is hinted, the case might be met by assuming for our system a position well outside the centre of a single universe, so that a fair proportion of its members, by virtue of their position, like planets at inferior conjunction, would appear to be moving in a different direction to the rest.

# CHAPTER XXXII

## STELLAR SPECTROSCOPY

A FULL account of the growth of stellar spectroscopy would take us too far from the purpose of this little book, and it is not easy to find a suitable place to consider it as a whole. It has been necessary to refer to spectroscopic inquiries in connection with various problems, and to quote such matters as spectral types, which should perhaps have been explained in an earlier chapter. It seems advisable to deal briefly with this matter in a separate chapter, instead of overloading the text with footnotes. We may here repeat the elementary propositions, implied in our notices of solar spectroscopy. A continuous spectrum infers incandescence of a body, either solid or divided into solid particles, or gaseous if non-transparent. A spectrum of bright lines denotes an incandescent gaseous transparent mass. If an incandescent body is surrounded by a cooler gaseous atmosphere, as in the case of the sun, we have the effect of absorption, the continuous spectrum being crossed by dark lines in the positions in which the surrounding gas, if alone, would show bright lines. A gaseous mass under pressure presents an irregular spectrum partly bright and partly shaded. It is evident, therefore, that the spectroscope cannot absolutely determine whether a body is solid or gaseous, and it is quite probable that stars similar to the sun are entirely gaseous, while the abnormal appearances caused by pressure give rise to considerable uncertainty in the interpretation of certain spectra, some lines being widened symmetrically, some unsymmetrically by being intensified at one side only, giving an appearance of shifting, some fringed and some suppressed altogether. Banded spectra are attributed to the presence of compounds.

The first classification of stellar spectra was due to Angelo Secchi in 1863, and was based principally on the prominence of blue, yellow, or red in the spectrum. His first and most numerous type contains white or bluish white stars, like Sirius and Vega, showing a coloured spectrum with broad bands of hydrogen and fine metallic lines in the brighter stars. Type II. consists of yellowish stars, as Capella and Arcturus, whose spectra resemble that of the sun. Type III. principally of red stars, as Antares, showing hazy bands and dark lines. Later Secchi distinguished a fourth type, deep red stars, with a spectrum consisting principally of a collection of lines in the yellow and red resembling a band, with another faint band in the blue and a bright one in the green. These are called Carbon stars. Wolf-Rayet stars, named from their co-discoverers at the Paris Observatory, were later still ascribed to a fifth type. Their spectra are composite as if a continuous spectrum had superposed on it an absorption and an emission spectrum, denoting glowing gas under peculiar pressure conditions. This is one of the types almost confined to the Milky Way.

H. C. Vogel of Potsdam, formerly of Bothkamp, introduced sub-divisions of these classes, merging the Wolf-Rayet stars into the second class.

Thus Ia is as described above for the first type.

Ib is similar, but the hydrogen lines are missing.

Ic shows both hydrogen and helium lines.

This class also is more frequent in the Milky Way. Similarly Class II. is in two divisions, the second containing the Wolf-Rayet stars. Class III. is also divided according as the bands are more prominent at the red or violet end. It will easily be inferred what is meant by Sirian, Solar, Antarian, and Helium stars.

A far more complete classification has been adopted for use in the Draper Memorial Catalogue, Miss Maury having recognised that the boundary between one class and another was often no more prominent than variations among stars ascribed to one class. This Harvard sub-division is more minute therefore, and the number of groups is twenty-two, each subdivided into classes. Five of the groups belong to a helium type called Orion stars, as most of the bright stars in that constellation belong to it; they are rich in hydrogen lines, but show also nearly a hundred others mostly not identified with any known elements. In addition to all these gradations are many stars assigned to a convenient temporary class, called stars with peculiar spectra. The recognition of characteristic spectra in the work of the Draper Catalogue is responsible for the large number of variables, and comparatively also of new stars, announced from time to time by Professor Pickering and Mrs Fleming.

But having reached this classification the next step is to interpret it, and here, as is often the case in partly speculative work, authorities differ. One order of development, assuming an original nebular condition, makes Orion or Helium stars the first stage, as they appear to be of low density and frequently have nebulous appendages; all seem enveloped in helium, but they are graded by Miss Maury, according as they show more and more decided traces of rather denser materials, as silicon, calcium and magnesium; a few also disclose oxygen absorption, but in general practically no "atmosphere" is present, as there is outside the sun's photosphere. Very little atmosphere is shown by the next group, Sirian stars, so their spectra are traversed by fewer lines than that of the sun; but those of hydrogen are so conspicuous that a star of this class, Vega, was the first to have its spectrum photographed by Huggins in 1879, disclosing a series of lines only completely obtained with difficulty in the laboratory by M. A. Cornu, using a strong spark through a tube of pure hydrogen. This set of lines was the first to be recognised as a series, with wave-lengths obeying some sort of law. If from a given point outside a given straight line, a set of other straight lines be drawn, each making with its neighbour the same small angle, they will intersect the given straight line in points whose consecutive distances increase faster and faster away from the foot of the perpendicular from the given point. Huggins' hydrogen series divides the spectrum in a similar manner, and Balmer's law published in 1885 is an empirical law approximately representing the numerical facts. Other series have been found for other substances, also obeying Balmer's law, and in particular a second series of hydrogen lines was picked out by Pickering in 1897 in the spectrum of ζ Puppis, adjudged to belong to the helium group. As this series has not yet been produced artificially, its identification rests on the fact that it is exactly similar to the Huggins series, just as in the case of a point and a line, a fresh series could be obtained by using a slightly different angle, starting from the same perpendicular.

This digression has rather taken us away from the Sirian type, but we may note the stages of evolution through various classes of this group by the relatively diminishing prominence of the hydrogen lines and the increasing show of metallic ones. Sirius itself shows an overwhelming preponderance of hydrogen lines, with a few "enhanced" lines of silicon and magnesium, and some others. The term "enhanced" is applied by Lockyer to lines which show more strongly in spark spectra than in arc spectra obtained from the same substance.

The next type, Solar stars, show hydrogen lines in a subordinate position compared with metallic lines, the sun itself showing only four hydrogen lines; and Capella, the acknowledged pattern of solar stars, owes its extra hydrogen lines in the violet to the companion star spectroscopically disdiscovered by Campbell and Newall in 1899, which belongs to a group between Sirian and solar stars, of which Procyon is an example. The calcium absorption is the strong feature of this class, and we have already noted its importance in the case of the sun, especially in regard to the prominences of the chromosphere. The existence of the "atmosphere" in these stars causes considerable variation from the exact solar type in some members of the group, such as Arcturus and especially Aldebaran.

Aldebaran, however, approximates to the group of red, or Antarian, stars, with "banded" spectra, remarkable for "flutings." A fluting is a set of lines like the Huggins series of hydrogen, but set very close together, giving a shaded appearance. Of the ten characteristic bands prevalent in this group, and long chemically unknown, Professor Fowler in 1904 identified eight as belonging to titanium or titanium oxide. These bands signify strong absorption, and it is this token of a dense atmosphere that distinguishes the group, the absorption being due to nine or ten metals, of which calcium and iron are the most prominent. About one in seven of this class is a variable. There is no hard and fast line to be drawn between helium and Sirian stars, or between Sirian and solar, so that it is generally surmised that the successive gradations show regular stages of evolution obligatory on all suns. But between solar stars and red stars the gradation is not so regular; there are "missing links." Moreover, Professor Hale considers that the carbon stars of the next class do not represent a stage beyond the Antarian, but an alternative line of evolution for solar stars. Hale himself, assisted by Ellerman, succeeded by 1903 in obtaining spectrograms of carbon stars, the difficulty caused by their weakness in photographically active rays being at last surmounted by using specially graded isochromatic plates for four successive sections of the spectrum, and by exposures ranging up to twenty-four hours in duration. A feature of their results consisted of cyanogen flutings in the blue part of the spectrum. They show many lines widened in sun-spots, but no enhanced lines. It is urged against Hale's theory of the evolution of carbon stars from solar stars that they are at a much greater distance, and on the whole in a different direction; since, as we have seen, they are very uncommon outside the Milky Way, while the opposite is the case with solar stars. Gaseous stars show bright and dark lines belonging to the same substances, the first of this class, $\gamma$ Cassiopeiæ, having been noted by Father Secchi in 1866, and soon followed by $\beta$ Lyræ, each showing a few bright hydrogen lines. A curious feature of these stars is the fact that the red C line of hydrogen, for instance, is sometimes bright and sometimes invisible, which, although recognised in $\beta$ Lyræ, a known variable, as being dependent on the phase of its light curve, is quite as conspicuous in $\gamma$ Cassiopeiæ, which is not regarded as a variable in the ordinary sense. The Wolf-Rayet stars, also gaseous, show no variability, either of light or spectrum; they resemble helium stars to some extent, but their helium lines are not conspicuous, and their affinity to the nebulæ is almost as great as to helium stars, so that they are sometimes regarded as an intermediate stage; a fundamental difference, however, being that the persistent bright lines in gaseous stars are one in the blue and one in the yellow, while in the nebulæ, as we have seen, these give place to a "nebulium" line in the green, and in spectroscopy yellow and blue do not produce green. In the valuable "Atlas of Representative Stellar Spectra," the fruit of many years of devoted labour by Sir William and Lady Huggins, which appeared in 1899, a scheme of evolution is given from comparatively cool nebulæ, growing hotter by degrees through progressive gradual condensation, so long as they remain entirely gaseous, until a maximum is reached, after which continued condensation tends to partial solidification, the gaseous constituents become proportionally less, the law of compensation becomes less and less applicable, and the temperature diminishes. Some theories used to start with an enormous temperature, but most now agree that this is not only unnecessary, but improbable. The chief differences between different "temperature" classifications are caused by the doubt as to which class really represents the hottest stars, and which groups belong to the cooling stage, and which to the earlier increase of temperature. In Lockyer's "Meteoritic Hypothesis," published in book-form in

1890, after appearing from 1887 onwards in a series of communications to the Royal Society, the evolution was traced from nebulæ through seven stages —gaseous, Antarian, imperfect solar, Sirian, true solar, carbon, and extinct. This arrangement, suggested before the distinction of helium stars as a separate group, makes Sirian stars the hottest, whereas Huggins' later series actually commences with Sirius. The plausible assumption from terrestrial analogy that white stars are hotter than yellow and yellow than red cannot be enforced, as the quality of the light received from a star is almost entirely controlled by the outer envelopes, which are at a totally different temperature to the interior mass. Other evidence, such as the low density of helium stars, is far more trustworthy, and is regarded as fixing helium stars as the first stage, not only by Huggins, but also by F. M'Clean, whose survey of the spectra of the bright stars in the northern hemisphere was supplemented by him with an extension to the south pole, obtained with the Victoria telescope which he presented to the Cape Observatory in 1897.

Lockyer himself has also considerably modified his first order of classification, and now agrees with most other systems in assigning the highest temperature to Orion stars. His latest classification suggests γ Argûs as a type of the hottest stars, the ascending series starting with Antares, and the descending series ending with Piscian stars (Type 19 Piscium), the former series including among other typical steps Aldebaran, α Cygni, and Rigel, while the latter includes Algol, Sirius, Procyon, and Arcturus. Gaps are, however, indicated in both.

In the case of unequal double stars, the frequency with which the "chief" star appears to belong to the solar, and the companion to the Sirian, type is explained by Miss Clerke on the hypothesis that in giant stars the greater effect of gravitation accelerates the evolution; so that, contrary to expectation, the primary has reached a later stage of development than the secondary. But though there is really no analogy with the solar system to justify this disappointed expectation, the researches of Lewis in the Struve Memoir already cited, tend to contradict, not the explanation, but the actual facts, by showing that the supposed "chief" star is in reality the secondary in many cases of the kind.

As regards the two red groups which, as we have seen, Hale considers to represent alternative tracks of development of solar stars, and which in Lockyer's scheme are of quite opposite tendencies, one rising to one solar type, while the other marks the fall from another, there is little analogy to help us; but the fact that in one class the flutings shade towards the red and in the other towards the violet, would seem to indicate that one is a case of rising, and the other of falling, temperature, were it not for the difficulty of accounting for the presence of compound substances in the earlier stages of evolution.

The more recent researches of Hartmann and Eberhard at Potsdam attribute excessive brightness not to transcendent temperature, but to "electrical luminescence." This pronouncement, rejecting the assumption involved in the ordinary view of "enhanced" lines, that the spark is hotter than the arc, is of considerable importance as emphasising the impracticability of explaining all types on a temperature basis only.

Dr W. E. Wilson of Daramona, Ireland, is another strong opponent of the "temperature" classification, and recent researches of A. W. Clayden on the subject of cloud-spheres and photospheres tend to show that similar effects can be produced by diminishing the mass instead of increasing the temperature; so that a large hot body might show precisely the same spectrum as a much cooler and less massive one. Such reasoning would go far to explain the inverted relation of brightness and mass of binary stars referred to by Lewis in the Struve Memoir.

The great interest and importance of research of this kind will be readily conceived from the most inadequate sketch, and it is not surprising that some of the world's greatest telescopes should be employed upon it. The first spectroscopic star catalogue was published by Vogel at Potsdam in 1883, containing over four thousand stars from one volume of Argelander's "Zones," from 2° south of the equator to 20° north of it. In the preparation of the Draper catalogue, 1890, the instrument used discarded the slit as unnecessary for stars, and substituted for the cylindrical lens then used to give width to the spectrum, an arrangement by which the star slowly crossed the field of view in a direction parallel to the edge of the prism by which its light was analysed. In 1899 the great Potsdam 31½-inch refractor was mounted, and spectrographs arranged for use with it. The Bruce spectrograph in conjunction with the 40-inch refractor of the Yerkes Observatory, in the hands, first of Hale and then of E. B. Frost and W. S. Adams, was at work about the same time. Campbell's observations with the Mills spectrograph on the 36-inch Lick refractor commenced earlier, in 1895, and the southern extension under W. H. Wright with the new Mills spectrograph despatched for two or three years to Santiago, Chili, was installed in 1904. Other recent installations include a spectrograph employed by V. M. Slipher in connection with the 24-inch Lowell telescope, and an elaborate thermostat to secure uniformity of temperature in the spectroscope attached to the 24-inch Victoria telescope at the Cape. With so many ardent workers in the field it is not surprising that progress in this comparatively new branch has been rapid. We have noted, for example, that the discoveries of spectroscopic binaries, commenced in 1889, reached 146 in 1905, and of these more than half were discovered with the Mills spectrographs at Lick or Santiago, 75 to be exact, the Yerkes Observatory, which started later, claiming 44. Moreover, progress is steadily being made in the direction of obtaining spectra of fainter stars, and side by side with this goes on the examination in detail of the spectrum of portions of the only star available for this purpose, the sun, and with them both, the necessary complement of the other branches, the spectroscopy of terrestrial substances in the laboratory. And with it all vast problems yet remain. The more we learn the more there is behind, and the more fully is recognised the necessity for co-operation, to lessen the waste of time involved in needless overlapping, to guard against the omission of sections, to ensure that economy with efficiency shall not be a mere parrot-cry. The principles of the Astrographic Conference, and of the International Solar Union, have already extended into stellar spectroscopy, which has not yet seen its jubilee.

# CHAPTER XXXIII

## CONCLUSION

WHERE, then, do we stand? Small creatures on a small planet, revolving about a not very big sun. Swift's often-misquoted lines—

> "So naturalists observe a flea
> Hath smaller fleas that on him prey;
> And these have smaller still to bite 'em,
> And so proceed *ad infinitum*"

—embody a rule that works equally well or badly both ways. In the case of the fleas, the upper limit is fairly obvious, but with the earth it is the lower limit that is familiar. Lambert, a contemporary of Laplace, speculated in the opposite direction, arguing by analogy from satellites to planets, and from planets to the sun, that our sun with other suns might be circling round some vaster orb, which with its compeers might form the family of a still mightier one, so on and on, if not *ad infinitum*, yet far beyond our powers of proof. No central suns were or are apparent, so Lambert conjectured them as possibly non-luminous. Dark bodies do exist as we have seen, and may be far more numerous than is usually supposed; they might frequently occult some distant star without our ever noticing it; their existence is sometimes suggested to account for the

apparent thinning of the stars in distant space, by those who doubt the finiteness of our universe.

But limiting our ideas to what we *can* see, there is room for much diversity of speculation.

Within this restricted region there certainly appear to be some vast stellar systems, far exceeding the dimensions of a simple binary.

Mizar, as we have seen, is a spectroscopic binary, and also a visual binary; Alcor, the rider, moves with it, and so apparently do all but the outermost two of the seven stars in the "Plough." Other systems are equally distinct, the famous Pleiades, for instance, and, judging from their similarity of spectra, most of the bright stars in Orion.

It does not now seem possible to regard small nebulæ as "island universes," in Herschel's phrase, distant galaxies as vast as the Milky Way. They seem rather, since Keeler's estimate, to represent the dispersed material of which the stars are made, and to which dead suns may yet return, as dust to dust, possibly disrupted by collision with other dead suns, after a short period of bright existence as temporary stars. Such a theory of Nova Persei and its nebulosity is not quite impossible, though strongly discredited on account of the velocities involved.

It may be urged against the theory of celestial evolution that each stage of growth should be equally represented, but this could not be expected unless the stages are of equal duration, which is in the last degree unlikely even during visibility. New stars are uncommon, and few, even dull red stars, are known to have disappeared, but this may only show the quickness of these stages compared with the millions of years demanded for the "life" of a star, which is by every mode of reasoning probably but a small part of its actual existence, for most of which it should be invisible. And in this invisibility may lie another view of the "finite" universe. We know from Newcomb's careful estimate that suns, except in recognised binary and multiple systems, do not cluster closely. He computes that only one is to be found on an average in every eight units of space, his unit being a sphere whose radius corresponds to a parallax of a second of arc, or more than three light-years. Thus a sphere of a diameter of some thirteen light-years[1] will contain on an average one visible star. But it has often been pointed out that any regular distribution of stars, however scanty, in *infinite* space must lead inevitably to the conclusion that some star must lie in every direction. It has been suggested that from the distant spaces light has not yet arrived, though on its way, but this is rather making an assumption as to the time of starting, for which no data are given. It seems, however, quite as effective to treat the matter quantitatively on the hypothesis of a short-life period.

Suppose, then, the visibility of a star to last for one per cent. of its existence (between one shining and the next, if we carry the evolution to its logical continuation). It follows that, if stars lie in every direction in space, there is only one chance in a hundred that, of a possibly infinite number of stars in a straight line, the nearest one should be shining; in other words, there is only one chance in a hundred that a star will be seen in any particular direction. The hundred is only given for convenience of illustration, a million would be equally probable or improbable, as we have no data by which to test it.

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[1] 13 light-years = 75,000,000,000,000 miles approximately.

This illustration is not given as a serious effort to combat the idea of a "finite" universe. It is a practical impossibility for the mind to grasp the conception of a universe finite or infinite. The intention is rather to show the inherent weakness of many abstract speculations. Some generalisations are definite and unmistakable. The visible universe is almost symmetrically divided by the Milky Way, where Helium and gaseous stars and nebulæ do congregate, while other nebulæ are rare, so that it may easily be regarded as the fundamental plane of the universe. We may be, as some maintain, practically in the centre of the space bounded by the galaxy. Even if so, it seems the wildest speculation to suggest with Dr Wallace that the solar system is the only system and the earth the only planet in that or any other system, fitted for the abode of man. Shifted a billion[1] miles in any direction, we should still be in the centre, but it is not so certain that we are.[2] Newcomb proposes a reference to the astrographic survey when completed, to decide this question. Easton, following out Keeler's pregnant suggestion, sees in its rifts and other irregularities evidence of spiral structure in the galaxy itself, with the solar system eccentrically placed between two successive wisps.

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[1] An English billion, *i.e.* a million millions.

[2] See p. 297.

It is also inferred—from the fact that stars of Secchi's first type are twice as frequent in the galactic zone as in the rest of the sky, while his second and third types are evenly distributed—that we are dealing with separate systems, hence Pickering and others treat them separately in regard to such problems as the determination of the solar apex, as we have had occasion to remark in previous chapters. It may also be that the whole cosmic cluster is not in the shape of a grindstone or lens, with its greatest extension round the plane of the galaxy. Its approximation to the ordinary spheroidal form was suggested by Radau twenty years ago. We cannot see stars further off in the galaxy than elsewhere, at least so far as we know. Stars without sensible parallax and stars on the limit of photographic visibility are found in all directions, and as a set-off against the preponderance of stars in the galaxy is that of nebulæ outside it, seeming to cluster round the points furthest from the plane. If this be so, we may regard the Milky Way as the equatorial zone of a rotating universe, and explain the thronging of brighter stars there on the analogy of the bulging equator of a rotating planet; or by the possible acceleration, due to increased velocity, of the evolution of clusters from nebulæ; or by the actual brightening of stars from the same cause, which also may modify or codify the distribution of certain types, yet leaving many subordinate systems within the confines of this "universal" globe to continue their almost independent motions. This is but speculation; none may read the riddle clearly; we can but "peep about," not necessarily "to find ourselves dishonourable graves," nor merely to magnify the scientific achievements of our fellow-men, but rather to marvel at the mighty works of a Supreme Intelligence, and to convince ourselves of our "colossal insignificance."

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